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ANNEX

A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

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VOL. XXV.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR
W. NICHOLSON,
CHARLOTTE STREET, BLOOMSBURY;
AND SOLD BY
J. STRATFORD, No. 112, HOLBORN HILL.

1810.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Sir George Cayley, Bart.; Mr. Acton; Mr. Robert Lyall; T. L. N.; W. Henry, M. D.; A. M.; N.; Mr. T. Sheldrake; T. LeGay Brewerton, Esq. F. R. Phys S. Edin.; Mr. P. Barlow; Francis Ellis, Esq.; W. N.; G. O.; A. B; J. Bostock, M. D.; J. F.; C.; Mr. R Winter; G. Cumberland, Esq.; T. Forster, Esq.; L. O. C; Mr. B. Cook; Rev. J. Blanchard; Dr. Clarke; J. Gough, Esq.

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The Engravings consist of 1. Figures 1, 4, 5, 6, and 7, (in one Quarto Plate) of Mr. Troughton's Apparatus for dividing Astronomical Instruments; 2. Figs. 2 and 3, of Mr. Troughton's Apparatus for dividing Astronomical Instruments; 3. Diagrams illustrating the Art of Aerial Navigation, by Sir George Cayley, Bart; 4. Leaf of the Mimosa Pudica, by Mr. Robert Lyall; 5. Sir George Cayley on Aerial Navigation; 6. Apparatus for procuring Potassium; 7. Dr. Wollaston's reflecting Goniometer; 8. The Intervertebral Joint of the Shark; 9. Mr. Miller's Drag for finding and raising the Body of a Person drowned; 10. His Reel Safeguard, for the Security of a Person going to the Assistance of one Drowning; 11. His Missile Rope to be flung to a Person in Danger of being drowned; 12. Apparatus of Messrs. Allen and Pepys, for the Respiration of Gasses by Animals; 13. Lieutenant Spratt's Homograph; 14. Mr. White's improved File for Letters, Receipts, &c.; 15. Diagrams illustrative of certain Properties of Solids, by John Gough, Esq.; 16. Capt. W. Bolton's Method of rigging his Jury Mast, and setting up a Ship's lower Rigging.

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A

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OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY, 1810.

ARTICLE I.

An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the uſual Tools for graduating are not employed; the whole Operation being so contrived, that no Errour can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities. By Mr. Edward Troughton. *Communicated by the Astronomer Royal*[*].

Division of astronomical instruments.

IT would ill become me, in addressing myself to the members of this society upon a subject which they are so well enabled to appreciate, to arrogate to myself more than may be assigned as my due, for whatever of success may have been the result of my long continued endeavours, exerted in prosecuting towards perfection *the dividing of Instruments immediately subservient to the purposes of astronomy.* A man very naturally will set a value upon a thing, on which so much of his life has been expended; and shall readily, therefore, be pardoned for saying, that con sidering some attainments which I have made on this sub ject as too valuable to be lost, and being encouraged also b the degree of attention which the Royal Society has eve paid to practical subjects, I feel myself ambitious of pre senting them to the public through what I deem the mos respectable channel in the world.

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[*] Philos. Trans. for 1809, p. 105.

The author's attention early turned to it.

It was as early as the year 1775, being then apprentice t my brother, the late Mr. John Troughton, that the art o dividing had become interesting to me; the study of as tronomy was also new and fascinating; and I then forme the resolution to aim at the nicer parts of my profession.

His brother skilful in it.

At the time alluded to, my brother, in the art of dividing was justly considered the rival of Ramsden; but he wa then almost unknown beyond the narrow circle of the ma thematical and optical instrument makers; for whom h was chiefly occupied in the division, by hand, of small as tronomical quadrants, and Hadley's sextants of large radius Notwithstanding my own employment at that time was of much inferior nature, yet I closely inspected his work, and tried at leisure hours on waste materials to imitate it. Witl as steady a hand, and as good an eye, as young men gene rally have, I was much disappointed at finding, that afte having made two points, neat and small to my liking, I could not bisect the distance between them, without enlarging, displacing, or deforming them with the points o the compasses. This circumstance gave me an early dislike to the tools then in use; and occasioned me the more uneasiness, as I foresaw that it was an evil which no practice, care, or habit could entirely cure: beam-compasses, spring-dividers, and a scale of equal parts, in short, appeared to me little better than so many sources of mischief.

Defect of the common practice.

Turning supposed to be nearest perfection.

I had already acquired a good share of dexterity, as a general workman. Of the different branches of our art, that of *turning* alone seemed to me to border on perfection. This juvenile conceit, fallacious as I afterwards found it, furnished the first train of thoughts, which led to the method about to be described; for it occurred to me, that if I could

Application of a roller to marking divisions.

by any means apply the principle of turning to the art of dividing instruments, the tools liable to objection might be dispensed with. The means of doing this were first suggested by seeing the action of the perambulator, or measuring wheel; the surface of the Earth presenting itself as the edge of the instrument to be divided, and the wheel of the perambulator as a narrow roller acting on that edge; and hence arose an idea, that some easy contrivance might be devised, for marking off the revolutions and parts of the roller upon the instrument. Since the year above-mentioned, several persons have proposed to me, as new, dividing by the roller, and I have been told, that it also occurred long ago to Hook, Sisson, and others; but, as Hatton on watch-making says, "I do not consider the man an inventor, who merely thinks of a thing; to be an inventor, in my opinion, he must act successfully upon the thought, so as to make it useful." I had no occasion, however, to have made an apology for acting upon a thought, which, unknown to me, had been previously conceived by others; for it will be seen in the sequel, how little the roller has to do in the result, and with what extreme caution it is found necessary to employ it.

When most accurate, defective.

When a roller is properly proportioned to the radius of the circle to be divided, and with its edge made a small matter conical, so that one side may be too great, and the other side too little, it may be adjusted so exactly, that it may be carried several times around the circle, without the errour of a single second; and it acts with so much steadiness, that it may not unaptly be considered as a wheel and pinion of indefinitely high numbers. Yet, such is the imperfection of the edges of the circle and roller, that, when worked with the greatest care, the intermediate parts, on a radius of two feet, will sometimes be unequal to the value of half a minute or more. After having found the terminating point of a quadrant or circle so permanent, although I was not prepared to expect perfect equality throughout, yet I was much mortified to find the errours so great, at least ten times as much as I expected; which fact indicated, beyond a doubt, that if the roller is to be trusted at all, it must only be trusted through a very short arc. Had there been any

Must be confined to short arcs.

thing slippery in the action, which would have been indicated by measuring the same part at different times differently, there would have been an end of it at once; but this not being the case in any sensible degree, the rolle becomes a useful auxiliary to fill up short intervals, the limits of which have been corrected by more certai means *.

Bird,

Porosity of the metal a source of errour.

* There are two things in the foregoing account of the action of the roller, which have a tendency to excite surprise. The first is, that the roller should, in different parts of its journey round the circle, measure the latter so differently. One would not wonder, however, if in taking the measure across a ploughed field, it should be found different to a parallel measure taken upon a gravel walk; and, in my opinion, the cases are not very dissimilar. Porosity of the metal, in one part of the circle more than in the other, must evidently have the same effect; brass unhammered is always porous; and the part, which has felt the effect of two blows, cannot be so dense as other parts which have felt the effect of three; and, should the edge of the circle be indented by *jarring turning*, it would produce a visible simillitude to ploughed ground. Every workman must be sufficiently upon his guard against such a palpable source of errour; yet perhaps with our greatest care we may not be able to avoid it altogether. The second is, that notwithstanding the inequality abovementioned, the roller having reached the point upon the circle from which it set out, should perform a second, third, &c., course of revolutions, without any sensible deviation from its former track; this is not perhaps so easily accounted for. It must be mentioned, that the exterior border of the circle should be *turned rounding*, presenting to the roller a convex edge, the radius of curvature of which is not greater than one tenth of an inch. Now, were the materials perfectly inelastic and impenetrable, the roller could only touch the circle in a *point*, and in passing round the circle, it could only occupy a *line* of contact. This in practice is not the case; the circle always marks the roller with a broad list, and thereby shows, that there is a yielding between them to a considerable amount. The breadth of this list is not less than one fiftieth of an inch; and it follows, that at least 12° of the circle's edge must be in contact at the same time; that the two surfaces yield to each other in depth, by a quantity equal to the *ver. sin.* of half that arc, or 1-1800th of an inch: and that the circle has always hold of the roller by nearly 1° of the edge of the latter. Whoever has examined the surfaces of metals, which have rolled against each other, must have observed that peculiar kind of indentation, that always accompanies their action; and there can be no doubt, that the particles of a roller, and those of the surface on which it acts, which mutually indent each other, will, upon a second

The roller and metal to be divided yield to each other.

course

Bird's method of division.

Bird, who enjoyed the undisputed reputation of being the most accurate divider of the age in which he lived, was the first who contrived the means how to render the usual divisions of the quadrant bisectional; which property, except his being unusually careful in avoiding the effects of unequal expansion from change of temperature, chiefly distinguished his method from others who divided by hand. This desirable object he accomplished by the use which he made of a finely divided scale of equal parts. The thing aimed at was, to obtain a point upon the arc at the highest *bisectional number of divisions* from 0, which in his eight feet quadrants was 1024, = 85° 20′. The extent of the beam compasses, with which he traced the arc upon the limb of the instrument to be divided, being set off upon that arc, gave the points 0° and 60°; which, being bisected, gave 30° more to complete the total arc. A second order of bisections gave points at 15° distance from each other; but that which denoted 75° was most useful. Now, from the known length of the radius, as measured upon the scale, the length of the chord of 10° 20′ was computed, taken off from the scale, and protracted from 75° forwards; and the chord of 4° 40′, being ascertained in the same manner, was set off from 90° backwards, meeting the chord of 10° 20′ in the continually bisectional arc of 85° 20′. This point being found, the work was carried on by bisections, and the chords, as they became small enough, were set off beyond this point, to supply the remainder of the quadrantal arc. My brother, whom I mentioned before, from mere want of a scale of equal parts upon which he could rely, contrived

Mr. John Troughton's method of di-

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course begun from the same point, indent each other deeper: this is not, however, exactly the case in question; for, whatever of fitting might have taken place between the surfaces of our roller and circle in the first revolution of the former, we should imagine would be obliterated by the fifteen turns which it must repeat over fresh ground. Experience shows, however, as every one will find who tries the experiment with good work, that on coming round to the point of commencement, the roller has the disposition to regain its former track; for, were this not the case, although the commensurate diameters were adjusted so exactly as to be without sensible errour in one course, yet a less errour than that which is so would become visible, when repeated through many courses.

viding bisectionally without a scale.

the means of dividing bisectionally without one. His method I will briefly state as follows, in the manner in whic it would apply to dividing a mural quadrant. The arcs o 60° and 30° give the total arc as before; and let the last ar of 30° be bisected, also the last arc of 15°, and again th last arc of 7° 30′: the two marks next 90° will now be 8 30′ and 86° 15′, consequently the point sought lies betwee them. Bisections will serve us no longer; but if we divid this space equally into three parts, the most forward of th two intermediate marks will give us 85°, and if we divid the portion of the arc between this mark and 86° 15′ als into three, the most backward of the two marks will denot 85° 25′. Lastly, if we divide any one of these last space into five, and set off one of these fifth parts backwards from 85° 25′, we shall have the desired point at 1024 divisions upon the arc from 0°. All the rest of the divisions which have been made in this operation, which I have called marks because they should be made as faint as possible, must be erased; for my brother would not suffer a mark to remain upon the arc to interfere with his future bisections.

Smeaton's preference of division by the computed chord.

Mr. Smeaton, in a paper to be more particularly noticed presently, justly remarks the want of a unity of principle in Mr. Bird's method; for he proceeds partly on the ground of the protracted radius, and partly upon that of the computed chord; which, as Smeaton observes, may or may not agree. Bird, without doubt, used the radius and its parts in order to secure an exact quadrant; but Smeaton, treating exactness in the total arc as of little value to astronomy, would, in order to secure the more essential property of equality of division, reject the radius altogether, and proceed entirely upon the simple principle of the computed chord.

Advantages of Mr. John Troughton's method.

The means pursued by my brother, to reach the point which terminates the great bisectional arc, is the only part in which it differs from Bird's method; and I think it is without prejudice, that I give it the preference. It is obvious, that it is as well calculated to procure equality of division, as the means suggested by Smeaton; at the same time that it is equal to Bird's in securing the precise measure of the total arc. It proceeds entirely upon the principle of the protracted structed chord of 60° and its subdivision; and the uncertainty, which is introduced into the work by the sparing use which is made of subdivision by 3 and 5, is, in my opinion, likely to be much exceeded by the errours of a divided scale*, and those of the hand and eye, in taking off the computed chords, and applying them to the arc of the instrument to be divided.

Ramsden's well known method of dividing by the engine unites so much accuracy and facility, that a better can hardly be wished for: and I may venture to say, that it will never be superseded, in the divisions of instruments of moderate radii. It was well suited to the time in which it appeared; a time when the improvements made in nautical astronomy, and the growing commerce of our country, called for a number of reflecting instruments, which never could have been supplied, had it been necessary to have divided them by hand; however, as it only applies to small instruments, it hardly comes within the subject of this paper.

Ramsden's method by the engine.

The method of Hindley, as described by Smeaton †, I will venture to predict will never be put in practice for dividing astronomical instruments, however applicable it might formerly have been for obtaining numbers for cutting clockwork, for which purpose it was originally intended. It consists of a train of violent operations with blunt tools, any one of which is sufficient to stretch the materials beyond, or press them within their natural state of rest; and, although the whole is done by contact, the nature of this contact is such, as, I think, ought rather to have been contrasted with, than represented as being similar to, the nature of the contact used in Smeaton's Pyrometer, which latter is performed by the most delicate touch; and is represented, I believe justly, to be sensible to the $\frac{1}{23040}$ part of an inch. Smeaton has, however, acquitted himself well, in describing and improving the method of his friend; and the world is particularly obliged to him for the historical

Hindley's method.

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* That Bird's scale was not without considerable errours, will be shown towards the end of this paper.

† Phil. Trans. for 1786.

part of his paper, as it contains valuable information, which perhaps no one else could have written.

London practice of dividing large instruments.

The only method of dividing large instruments now practised in London, that I know of, beside my own, has not yet, I believe, been made public. It consists in dividing by hand with beam compasses and spring dividers, in the usual way; with the addition of examining the work by microscopes, and correcting it, as it proceeds, by pressing forwards or backwards by hand, with a fine conical point, those dots which appear erroneous; and thus adjusting them to their proper places. The method admits of considerable accuracy, provided the operator has a steady hand and good eye; but his work will ever be irregular and inelegant. He must have a circular line passing through the middle of his dots, to enable him to make and keep them at an equal distance from the centre. The bisectional arcs, also, which cut them across, deform them much; and, what is worse, the dots which require correction (about two thirds perhaps of the whole) will become larger than the rest, and unequally so in proportion to the number of attempts, which have been found necessary to adjust them. In the course of which operation, some of them grow insufferably too large, and it becomes necessary to reduce them to an equality with their neighbours. This is done with the burnisher, and causes a hollow in the surface, which has a very disagreeable appearance. Moreover, dots which have been burnished up are always ill defined, and of a bad figure. Sir George Shuckburgh Evelyn, in his paper on the Equatorial*, denominates these "doubtful or bad points;" and (considering the few places which he examines) they bear no inconsiderable proportion to the whole. In my opinion, it would be a great improvement of this method, to divide the whole by hand at once, and afterward to correct the whole; for a dot forced to its place, as above, will seldom allow the compass-point to rest in the centre of its apparent area; therefore other dots made from these will scarcely ever be found in their true places. This improvement also prevents the corrected dots from being injured, or moved,

Its defects.

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* Phil. Trans for 1793.

by the future application of the compasses, no such application being necessary.

Its

I will now dismiss this method of dividing, with observing, that it is tedious in the extreme; and did I not know the contrary beyond a doubt, I should have supposed it to have surpassed the utmost limit of human patience*. When I made my first essay at subdividing with the roller, I used this method, according to the improvement suggested above, of correcting a few primitive points; but even this was too slow for one who had too much to do. Perhaps, however, had my instruments been divided for me by an assistant, I might not have grudged to have paid him for the labour of going through the whole work by the method of adjustment; nor have felt the necessity of contriving a better way.

Mr. Ed
Trough
method

I might now extend the account of my method of dividing to a great length; by relating the alterations which the apparatus has undergone during a long course of years†, and the various manner of its application, before I brought it to its present state of improvement; but I think I may save myself this trouble, for truly I do not see its use: I will, therefore, proceed immediately to a disclosure of the method, as practised on a late occasion, in the dividing of a four feet meridian circle, now the property of Stephen Groombridge, Esq., of Blackheath.

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* At the time alluded to, the double microscopic micrometer was unknown to me, and I did not learn its use, for these purposes, till the year 1790, from General Roy's description of the large theodolite. Previous to that time, I had used a frame, which carried a single wire very near the surface to be divided; this wire was movable by a fine micrometer screw, and was viewed by a single lens inserted in the lower end of a tube, which, for the purpose of taking off the parallax, was 4 inches long. The greatest objection to this mode of constructing the apparatus is, that the wire, being necessarily exposed, is apt to gather up the dust; yet it is preferable to the one now in use, in cases where any doubt is entertained of the accuracy of the plane which is to receive the divisions.

† The full conception of the method had occupied my mind in the year 1778; but as my brother could not be readily persuaded to relinquish a branch of the business to me in which he himself excelled, it was not until September 1785 that I produced my first specimen, by dividing an astronomical quadrant of two feet radius.

Described.

The surface of the circle which is to receive the divisions, as well as its inner and outer edges, but especially the latter, should be turned in the most exact and careful manner; the reason for which will be better understood, when we come to describe the mode of applying the roller: and, as no projection can be admitted beyond the limb, if the telescope, as is generally the case, be longer than the diameter, those parts which extend farther must be so applied, that they may be removed during the operation of dividing. Fig. 1, Pl. I, and 2, Pl. II, represent the principal parts of the apparatus; Fig. 1 showing the plan, and Fig. 2 the elevation; in both of which the same letters of reference are affixed to corresponding parts, and both are drawn to a scale of half dimensions. A A is a part of the circle, the surface of which is seen in the plan, and the edge is seen in the elevation. B B B is the main plate of the apparatus, resting with its four feet *a a a a* upon the surface of the arc; these feet, being screws, may be adjusted so as to take equal shares of the weight, and then are fastened by nuts below the plate, as shown in Fig. 2. C C and D D are two similar plates, each attached to the main plate, one above and the other below, by four pillars; and in them are centred the ends of the axis of the roller E. F and G are two friction wheels, the latter firmly fastened to B, but the former is fixed in an adjustable frame, by means of which adjustment these wheels and the roller E, may be made to press; the former on the interior, and the latter on the exterior edge of the circle, with an equal and convenient force*. At the extremities of the axis of the roller, and attached to the middle of the plates C and D, are two bridges, having a screw in each; by means of which an adjustment is procured for raising or lowering the roller respecting the edge

Apparatus.

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* Sufficient spring for keeping the roller in close and uniform contact with the edge of the circle is found in the apparatus, without any particular contrivance for this purpose; the bending of the pillars of the secondary frames and of the axis of the roller chiefly supplies this property.

of the circle, whereby the former, having its diameter at the upper edge about ·001 of an inch greater than at the lower edge (being, as before described, a little conical), it may easily be brought to the position where it will measure the proper portion of the circle.

The roller.

Much experience and thought upon the subject have taught me, that the roller should be equal to one sixteenth part of the circle to be divided, or that it should revolve once in 22° 30′; and that the roller itself should be divided into sixteen parts; no matter whether with absolute truth, for accuracy is not at all essential here. Each of such divisions of the roller will correspond with an angle upon the circle of 1° 24′ 22·5″, or $\frac{1}{256}$th part of the circle. This number of principal divisions was chosen, on account of its being capable of continual bisection; but they do not fall in with the ultimate divisions of the circle, which are intended to be equal to 5′ each.

Microscopes.

The next thing to be considered is, how to make the roller measure the circle. As two microscopes are here necessary, and those which I use are very simple, I will in this place give a description of them. Fig. 6 is a section of the full size, and sufficiently explains their construction, and the position of the glasses; but the micrometer part and manner of mounting it are better shown at H, in Fig. 1 and 2.

Micrometer.

The micrometer part consists of an oblong square frame, which is soldered into a slit, cut at right angles in the main tube; another similar piece nicely fitted into the former, and having a small motion at right angles to the axis of the microscope, has at one end a cylindrical guide pin, and at the other a micrometer screw; a spring of steel wire is also applied, as seen in the section, to prevent play, by keeping the head of the micrometer in close contact with the fixed frame. This head is divided into one hundred parts, which are numbered each way to 50; the use of which will be shown hereafter. A fine wire is stretched across the movable frame, for the purpose of bisecting fine dots. Two of these microscopes are necessary; also a third, which need not have the divided head, and must have in the movable frame two wires crossing each other at an angle of about 30°; this microscope is shown at I, Fig. 1. In the two first micrometers,

meters, a division of the head is of the value of about 0·2″ and the power and distinctness such, that when great care is taken, a much greater errour than to the amount of one of these divisions cannot well be committed in setting the wire across the image of a well made dot. The double eye glass has a motion by hand, for producing distinct vision of the wire; and distinct vision of the dots is procured by similar adjustment of the whole microscope.

Adjustment of the size of the roller.

Use & adaptation of the two microscopes.

The first step towards sizing the roller is, to compute its diameter according to the measure of the circle, and to reduce it agreeably thereto, taking care to leave it a small matter too large. The second step is, after having brought the roller into its place in the plate B B, to make a mark upon the surface of the circle near the edge, and a similar one upon the roller, exactly opposite each other; then carry the apparatus forward with a steady hand, until the roller has made sixteen revolutions. If, now, the mark upon the roller, by having overreached the one upon the circle, shows it to be much too large, take it out of the frame, and reduce it by turning accordingly. When, by repeating this, it is found to be very near, it may be turned about ·001 of an inch smaller on the lower edge, and so far its preparation is completed. The third and last step is, the use and adaptation of the two microscopes; one of these must take its position at H in Fig. 1, viewing a small well defined dot made for the purpose on the circle; the other, not represented in the figure, must also be fixed to the main plate of Fig. 1, as near to the former as possible, but viewing one of the divisions on the roller. With a due attention to each microscope, it will now be seen to the greatest exactness, when, by raising or depressing the roller, its commensurate diameter is found.

Apparatus for making the dots in the circle.

Fig. 3, Pl. II, is a representation of the apparatus for transferring the divisions of the roller to the circle. It consists of two slender bars, which, being seen edgewise in the figure, have only the appearance of narrow lines; but, when looked at from above, they resemble the form of the letter A. They are fastened to the main frame, as at W and Z, by short pillars, having also the off leg of the angle secured in the same manner; Y is a fine conical steel point for making the dots, and X is a feeler, whereby the point Y may be pressed down with a uniform force, which force may be adjusted, by bending the end of the bar just above the point, so as to make the dots of the proper size. The point Y yields most readily to a perpendicular action; but is amply secured against any eccentric or lateral deviation.

Primary dots.

The apparatus, so far described, is complete for laying our foundation, *i.e.* making 256 primary dots; no matter whether with perfect truth, or not, as was said respecting the divisions of the roller; precision in either is not to be expected, or wished; but it is of some importance, that they should be all of the same size, concentric, small, and round. They should occupy a position very near the extreme border of the circle, as well to give them the greatest radius possible, as that there should be room for the stationary microscope and other mechanism, which will be described hereafter.

It must be noticed, that there is a clamp and adjusting screw attached to the main plate of Fig. 1; but, as it differs in no respect from the usual contrivances for quick and slow motion, it has been judged unnecessary to incumber the drawing with it.

Method of making them.

Now, the roller having been adjusted, with one microscope H upon its proper dot on the circle, and the other microscope at the first division on the roller; place the apparatus of Fig. 3 so that the dotting point Y may stand directly over the place which is designed for the beginning of the divisions. In this position of things, let the feeler X be pressed down, until its lower end comes into contact with the circle; this will carry down the point, and make the first impression, or primary dot, upon the circle; unclamp the apparatus, and carry it forwards by hand, until another division of the roller comes near the wire of the microscope; then clamp it, and with the screw motion make the coincidence complete; where again press upon the feeler for the second dot: proceed in this manner until the whole round is completed.

Errours of these dots to be ascertained.

From these 256 erroneous divisions, by a certain course of examination, and by computation, to ascertain their absolute and individual errours, and to form these errours into convenient tables, is the next part of the process, and makes a very important branch of my method of dividing.

Method of doing this.

The apparatus must now be taken off, and the circle mounted in the same manner, as it will be in the observatory. The two microscopes, which have divided heads, must also be firmly fixed to the support of the instrument, on opposite sides, and their wires brought to bisect the first dot, and the one which should be 180° distant. Now, the microscopes remaining fixed, turn the circle half round, or until the first microscope coincides with the opposite dot; and, if the other microscope be exactly at the other dot, it is obvious that these dots are 180° apart, or in the true diameter of the circle; and if they disagree, it is obvious, that half the quantity by which they disagree, as measured by the divisions of the micrometer head, is the errour of the opposite division; for the quantity measured is that by which the greater portion of the circle exceeds the less. It is convenient to note these errours + or —, as the dots are found too forward or too backward, according to the numbering of the degrees; and for the purpose of distinguishing the + and — errours, the heads, as mentioned before, are numbered backwards and forwards to fifty. One of the microscopes remaining as before, remove the other to a position at right angles; and, considering for the present both the former dots to be true, examine the other by them; *i. e.* as before, try by the micrometer how many divisions of the head the greater half of the semicircle exceeds the less, and note half the quantity + or —, as before, and do the same for the other semicircle. One of the micrometers must now be set at an angle of 45° with the other, and the half-differences of the two parts of each of the four quadrants registered with their respective signs. When the circle is a vertical one, as in the present instance, it is much the best to proceed so far in the examination with it in that position, for fear of any general bending or spring of the figure; but, for the examination of smaller arcs than 45°, it will be perfectly safe, and more convenient, to have it horizontal; because the dividing apparatus will then carry the micrometers, several perforations being made in the plate B for the limb to be seen through at proper intervals. The micrometers must now be placed at a distance of 22° 30′, and the half differences of the parts of all the arcs of 45° measured and noted as before; thus descending by bisections to 11° 15′, 5° 37′ 30″, and 2° 48′ 45″. Half this last quantity is too small to allow the micrometers to be brought near enough; but it will have the desired effect, if they are placed at that quantity and its half, *i. e.* 4° 13′ 7·5″; in which case the examination, instead of being made at the next, will take place at the next division but one, to that which is the subject of trial. During the whole of the time that the examination is made, all the dots, except the one under examination, are for the present supposed to be in their true places; and the only thing in this most important part of the business, from first to last, is to ascertain with the utmost care, in divisions of the micrometer head, how much one of the parts of the interval under examination exceeds the other, and carefully to tabulate the half of their difference.

The instrument should be engraved before it is divided.

I will suppose that every one, who attempts to divide a large astronomical instrument, will have it engraved first. Dividing is a most delicate operation, and every coarser one should precede it. Besides, its being numbered is particularly useful to distinguish one dot from another; thus, in the two annexed tables of errours, the side columns give significant names to every dot, in terms of its value to the nearest tenth of a degree, and the mistaking of one for another is rendered nearly impossible.

Tables of apparent errours.

The foregoing examination furnishes materials for the construction of the table of half differences, or apparent errours*. The first line of this table consists of two varieties; *i. e.* the micrometers were at 180° distance for obtaining the numbers which fill the columns of the first and third quadrant; and at 90°, for those of the second and fourth quadrant. The third variety makes one line, and was obtained with a distance of 45°: the fourth consists of two lines, with a distance of 22° 30′: the fifth of four lines, with a disa distance of 11° 15′: the sixth of eight lines, with a distance of 5° 37′ 30″: the seventh of sixteen lines, with a distance of 2° 48′ 45″: and the eighth and last variety, being the remainder of the table, consist of thirty two lines, and was obtained with a distance of 4° 13′ 7·5″.

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* If the table of real errours be computed as the work of examination proceeds, there will be no occasion for this table at all; but I think it best not to let one part interfere with another, and therefore I examine the whole before I begin to compute.

Table of real errours.

The table of apparent errours, or half differences, just explained, furnishes data for computing the table of real errours. The rule is this; let a be the real errour of the preceding dot, and b that of the following one, and c the apparent errour, taken from the table of half differences, of the dot under investigation; then is $\frac{a+b}{2} + c =$ its real errour. But, as this simple expression may not be so generally understood by workmen as I wish, it may be necessary to say the same thing less concisely. If the real errours of the preceding and following dots are both +, or both —, take half their sum, and prefix thereto the common sign; but, if one of them is +, and the other —, take half their difference, prefixing the sign of the greater quantity: again, if the apparent errour of the dot under investigation has the same sign of the quantity found above, give to their sum the common sign, for the real errour; but if their signs are contrary, give to their difference the sign of the greater for the real errour. I add a few examples.

Example. 1.

For the first point of the second quadrant.

Real errour of the first point of the first quadrant	—	0·0
Real errour of the first point of the third quadrant	—	6·9
Half sum or half difference	—	3·4
Apparent errour of the dot under trial	+	12·2
Real errour	+	8·8

Example 2.

For the point 45° of the second quadrant.

Real errour of the first point of the quadrant	+	8·8
Real errour of the last point of the quadrant	—	6·9
Half difference	+	0·9
Apparent errour of the dot under trial	—	8·9
Real errour	—	8·0

Example 3.

Point 89°·6, or last point, of the third quadrant

Real errour of the point 84°·4 of the third quadrant	— 21·0
Real errour of the point 2°·8 of the fourth quadrant	— 2·9
Half sum	— 11·9
Apparent errour of the dot under trial	— 4·0
Real errour	— 15·9

Example 4.

Point 88°·6, or last, of the fourth quadrant.

Real errour of the point 84°·4 of the fourth quadrant	— 21·6
Real errour of the point 2°·8 of the first quadrant	— 10·2
Half sum	— 15·0
Apparent errour of the dot under trial	+ 9·5
Real errour	— 6·4

It is convenient, in the formation of the table of real errours, that they should be inserted in the order of the numbering of the degrees on their respective quadrants; although their computation necessarily took place in the order in which the examination was carried on, or according to the arrangement in the table of apparent errours. The first dot of the first quadrant having been assumed to be in its true place, the first of the third quadrant will err by just half the difference found by the examination; therefore these errours are alike in both tables. The real errour of the first dot of the second quadrant comes out in the first example; that of the fourth was found in like manner, and completes the first line. It is convenient to put the errour of the division 90° of each quadrant at the bottom of each column, although it is the same as the point 0° on the following quadrant. The line 45° is next filled up; the second example shows this; but there is no occasion to dwell longer upon this explanation; for every one, who is at all fit for such pursuits, will think what has already been said fully sufficient for his purpose. However, I will just mention that there can be no danger, in the formation of this table, of taking from a wrong line the real errours which are to be the criterion for finding that of the one under trial; because they are in the next line to it; the others, which intervene in the full table, not being yet inserted. The last course of all is, however, an exception; for, as the examining microscopes could not be brought near enough to bisect the angle 2° 48′ 45″, recourse was had to that quantity and its half; on which account the examination is prosecuted by using errours at two lines distance, as is shown in the two last examples.

All the computations should be preserved.

When the table of real errours is constructed, the other table, although it is of no farther use, should not be thrown away; for, if any material mistake has been committed, it will be discovered as the operation of dividing is carried on; and, in this case, the table of apparent errours must be had recourse to; indeed, not a figure should be deſtroyed until the work is done*.

Respecting the angular value of the numbers in these tables, it may be worth mentioning, that it is not of the least importance; 100 of them being comprised in one revolution of the micrometer screw; and, in the instance before me, 5·6 of them made no more than a second. It is not pretended, that one of these parts was seen beyond a doubt, being scarcely $\frac{1}{10000}$ of an inch, much less the tenths, as exhibited in the tables; but, as they were visible upon the micrometer heads, it was judged best to take them into the account.

(To be concluded in our next.)

II.

On Platina and native Palladium from Brasil. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S*†.

Platina not found in Europe till lately,

ALTHOUGH platina has now been known to mineralogists for more than sixty years, yet it had not been discovered in any other places than Choco and Santa Fé, whence it was originally brought, until about two years since Mr. Vauquelin discovered it in some gray silver ores from Guadalcanal in Estremadura. In analysing these ores, he found some fragments that contained as much as one tenth of their weight of platina, but he did not find it accompanied by any of the new metals, that have lately been discovered in the Peruvian ore of platina.

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* This is a very useful hint, applicable on many occasions. C.

† Philos. Trans. for 1809, p. 189.

and then not accompanied with the new metals.

Some from Brasil mixed with native palladium.

The specimen which I am now about to describe is derived from a third source, and it is rendered the more interesting by having grains of native palladium mixed with it. This new mineral has lately been received from the gold mines in Brasil, by H. E. Chev. de Souza Coutinho, ambassador from the court of Portugal, resident in this country; and I am in hopes that some account of it may be acceptable to the Royal Society, although the analysis must necessarily be very imperfect, from the small quantity to which my experiments have unavoidably been confined.

Appearance different from that of the common ore of platina.

The general aspect of this specimen is so different from the common ore of platina, that I could form no conjecture of what ingredients it might be found to consist. Its appearance was such indeed, as at first sight to induce a suspicion of its not being in a natural state, for it had very much the spongy form, which is given to platina from imperfect attempts to render it malleable by means of arsenic.

But not owing to art.

One circumstance, however, occasions a presumption, that no art has been employed in giving the grains their present appearance; as upon close inspection many small particles of gold are discernible, but there is none of the magnetic iron sand, with which the Peruvian ore abounds, nor any of the small hyacinths, which I have formerly noticed as accompanying that mineral*.

Common ore of platina described.

It is very well known, that the common ore of platina in general consists of flattened grains, that appear so much worn at their surface, as to be in a considerable degree polished, and the roughness observable in some of the larger grains arises from concave indentations of a reddish brown or black colour. The Brasilian platina, on the contrary, has

Brasilian.

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* Phil. Trans. for 1805, p. 318; or Journal, Vol. XIII, p. 119.

no polish, and does not appear worn; but most of the grains seem to be small fragments of a spongy substance; and even those which are yet entire and rounded on all sides present a sort of roughness totally different from that of the former, as their surface consists of small spherical protuberances closely coherent to each other, with the interstices extremely clean, and free from any degree of tarnish.

Analysed.

The first portion that I employed for solution was taken without any selection, and being digested with a small quantity of nitro-muriatic acid, two of the grains were acted on much more rapidly than is usual with platina, and seemed to give a redder colour than that metal alone. These grains were consequently taken out, washed, and reserved for separate examination, and the solution was allowed to proceed till the rest were entirely dissolved. By the addition of muriate of ammonia an abundant precipitate was formed of a bright yellow colour. This precipitate was evidently platina, and its colour satisfied me, that the grains had not been brought into their present state from Peruvian platina by means of arsenic; for where arsenic has been employed, I have observed that the iridium contained in that ore is rendered more soluble than before, and thence communicates its red colour to the precipitate.

No iridium.

From the grains thus examined, there appeared not to be any iridium dissolved, nor any black powder containing iridium undissolved.

Palladium.

I next endeavoured, by prussiate of mercury, to ascertain the presence of palladium; but though a precipitate which occurred indicated a certain quantity, it remained doubtful whether it was derived from the grains of platina themselves, or from the two small fragments that had been in part dissolved before they were separated from the rest.

No iron, or rhodium.

By addition of ammonia to the solution, no iron was precipitated; and when the solution was afterwards allowed slowly to evaporate, I could discern no crystals or colour that I could ascribe to the presence of rhodium. In short, it seemed that these grains are really native platina nearly pure.

A minute portion of gold.

In order to discover whether the grains themselves contained any portion of gold, I selected three of the largest weighing together eight grains and a half; and after a solution and precipitation, as before, by muriate of ammonia, I added a solution of green sulphate of iron, and obtained a precipitate of gold. It was, however, far too small in quantity to be estimated with correctness, but certainly did not exceed the $\frac{1}{100}$ of a grain. This, it is to be observed, is another circumstance in which the present mineral differs from the Peruvian ore of platina, which I believe never contains (in the ore itself) the smallest quantity of gold.

Palladium.

In this experiment also, I tried to detect the existence of palladium in the solution, and by prussiate of mercury again ascertained its presence; but it was in too small quantity for estimating the proportion it bore to the whole mass.

The gold alloyed with silver.

It may deserve to be remarked, that though neither the Peruvian nor Brasilian grains of platina contain any silver, yet the gold which accompanies them is in each instance so much alloyed with silver, that from about thirty small scales of gold picked from Peruvian platina, weighing two grains, I obtained as much as four tenths of a grain of silver, or one fifth part of their weight.

Native Palladium.

Native palladium.

The two fragments, that had been separated from the first solution, next claimed my attention, and evidently deserved a careful examination. They were each placed in a drop of nitric acid, and each communicated a deep red colour, which, by the tests of prussiate of mercury and green sulphate of iron, I was satisfied arose from palladium. The smaller fragment was then divided, and one portion allowed to remain in the acid till it seemed completely dissolved, and the other examined by the blowpipe. The utmost heat that could be given appeared to have no effect; but when a small piece of sulphur was applied to it, it fused instantly; by continuance of the heat, it parted with the sulphur, and became completely malleable. In short, it perfectly resembled palladium; and it retained its brilliancy in cooling, I judged it to be nearly pure.

Nearly pure.

Afforded a trace of iridium.

But as the surfaces which had been acted upon by nitric acid had a degree of blackness, that might be owing to some insoluble impurity, I have since that time dissolved the larger fragment for the sake of discovering the cause of this appearance. Hot nitric acid dissolved by far the greatest part; but there remained a black powder on which a fresh addition of this acid alone had no farther effect. But when a drop or two of muriatic acid was added, the whole was very soon dissolved. By the addition of muriate of ammonia, it became evident from the precipitate, that the residuum was principally platina. But this precipitate, instead of being yellow, had the deep red colour, which is usually occasioned by the presence of iridium. The platina reduced from this precipitate was also too black for pure platina, and when it was again dissolved, the solution was of a deep red, and the precipitate by muriate of ammonia red, as before; so that although the grains of Brasilian platina appear to be free from iridium, as well as from many other impurities that form part of the Peruvian ore, yet the grains of native palladium that accompany them afford a trace of this ingredient, and occasion a presumption, that osmium and rhodium may hereafter appear, when we can obtain this mineral in larger quantity.

Perhaps too may contain osmium, and rhodium.

Since the whole weight of metal employed in the last experiment did not exceed 1$\frac{3}{10}$ grain, it is in vain to attempt to estimate the proportion of the ingredients, but if I am near the truth, in considering the quantity of the red precipitate as about one fifth of a grain, of which less than half is platina, those who are best acquainted with the intense colouring power of iridium may endeavour to form a conception of the extremely small quantity that can be present.

Palladium distinguishable by fibres diverging from one extremity.

As soon as I had ascertained the existence of native palladium, I endeavoured, by examination of its external characters, to distinguish its appearance from that of the surrounding substances, and I found it by no means difficult, although no difference of colour could be discerned. Having remarked that the larger fragment appeared rather fibrous, and that the fibres were in some degree divergent from one extremity, I examined the remainder of the small specimen which had originally been given to me, and by this peculiarity of structure I soon detected a third fragment, which upon trial proved to be the same substance.

By favour of the Chev. de Souza I was also permitted, with this view, to examine the specimen which remained in his possession, and had soon the satisfaction of discovering two more fragments of the same mineral, and as I was in no one instance deceived in my choice, by attending to the radiating fibres, I am in hopes that this external character will enable persons to distinguish this metal, in situations where they have not an opportunity of deciding by chemical experiment.

III.

On the Identity of Columbium and Tantalum. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S*[*].

Discovery of columbium and tantalum.

Their identity suspected.

WITHIN a short time after the discovery of columbium by Mr. Hatchett, in 1801[†], a metallic substance was also discovered in Sweden by Mr. Eckeberg[‡], differing from every metal then known to him; and accordingly he described the properties by which it might be distinguished from those which it most nearly resembled. But although the Swedish metal has retained the name of tantalum given to it by Mr. Eckeberg, a reasonable degree of doubt has been entertained by chemists, whether these two authors had not in fact described the same substance; and it has been regretted, that the discoverers themselves, who would have been most able to remove the uncertainty, had not had opportunities of comparing their respective minerals, or the products of their analyses.

Tantalum compared with oxide of columbium.

As I have lately obtained small specimens of the two Swedish minerals, tantalite and yttro-tantalite, from which I could obtain tantalum, and was very desirous of comparing its properties with those of columbium, Mr. Hatchett very obligingly furnished me with some oxide of the latter which remained in his possession.

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[*] Philosophical Transactions for 1809, p. 246.

[†] Phil. Trans. for 1802: or Journal, vol. II, p. 129, 176.

[‡] *Vetenskaps Academiens Handlingar*, 1802, *p.* 68. *Journal des Mines*, Vol. XII, p. 245. or, *Journal*, Vol. III, p. 251.]

Considerable resemblance.

The resemblance was such in my first trials, as to induce me to endeavour to procure a farther supply of columbium; and by application to the Trustees of the British Museum, I was allowed to detach a few grains from the original specimen analysed by Mr. Hatchett.

The American and Swedish minerals much alike,

Notwithstanding the quantity employed in my analyses was thus limited, I have, nevertheless, by proportionate economy of the materials, been enabled to render my experiments sufficiently numerous, and have found so many points of agreement in the modes by which each of these bodies can or cannot be dissolved or precipitated, as to prove very satisfactorily, that these American and Swedish specimens in fact contain the same metal; and since the reagents I have employed are in the hands of every chemist, the properties which I shall enumerate are such as will be most useful in the practical examination of any other minerals, in which this metal may be found to occur.

In external appearance,

In appearance the columbite is so like tantalite, that it is extremely difficult to discern a difference, that can be relied upon. The external surface, as well as the colour and lustre of the fracture, are precisely the same; but columbite breaks rather more easily by a blow, and the fracture of it is less uniform, appearing in some parts irregularly shattered; nevertheless, when the two are rubbed against each other, the hardness appears to be the same, and the colour of the scratch has the same tint of very dark brown.

and component parts.

By analysis also, these bodies are found to consist of the same three ingredients; a white oxide, combined with iron and manganese.

Treated with alkalis.

Either of these minerals, when reduced to powder, is very readily acted upon by potash; but as the iron contained in them is not affected by alkalis, it appeared better to add a small proportion of borax.

Five grains of columbite, being mixed with twenty-five grains of carbonate of potash and ten grains of borax were fused together for a few minutes, and found to be perfectly incorporated. The colour was of a deep green, from the quantity of manganese present. The mass when cold could be softened with water, and a portion of the oxide could be so dissolved; but it seemed preferable to employ dilute muriatic acid, which by dissolving all the other ingredients excepting columbium, left the oxide nearly white, by the removal of the iron and manganese that had been combined with it.

The muriatic solution having been poured off and neutralized with carbonate of ammonia, the iron was then separated by succinate of ammonia; after which the manganese was precipitated by prussiate of potash.

Products of columbite.

The products thus obtained from five grains of columbite, after each had been heated to redness, were nearly,

White oxide	4 grains
Oxide of iron	¼
Oxide of manganese	¼

but it cannot be supposed that *proportions* deduced from experiments made on so small a scale can be entirely depended upon, although the *properties* of bodies may be so discerned, nearly as well as when larger quantities are employed.

Products of tantalite.

An equal weight of tantalite taken from a specimen, of which the specific gravity was 7·8, yielded, by the same treatment,

White oxide	4¼ grains
Oxide of iron	½
Oxide of manganese	$\frac{2}{10}$

The insolubility of the oxide in mineral acids.

The white oxides obtained from each of these minerals are remarkable for their insolubility in the three common mineral acids, as both Mr. Hatchett and Mr. Eckeberg have observed.

In muriatic acid they cannot be said to be absolutely insoluble; but they are not sufficiently soluble for the purposes of analysis.

In nitric acid they are also nearly, if not perfectly insoluble.

In sulphuric acid, when concentrated and boiling, the oxide of columbium may be dissolved in small quantity, and so also may the oxide obtained from tantalite.

Potash renders it soluble in water.

The proper solvent, as has been observed by Mr. Hatchet and Mr. Eckeberg, is potash; and as it is not required to b in its caustic state, I employed the crystallized carbonate o potash on account of its purity and uniformity. Of thi salt about eight grains seemed requisite to be fused with on of the oxide obtained from either of these minerals to render it soluble in water.

Soda does not, except in large proportion, and with heat.

Soda also combines with the oxide, and may be said t dissolve it; but a far greater proportion of this alkali is necessary, and a larger quantity of water. And although solution may have been effected that is transparent whil hot, it very soon becomes opaque in cooling, and finally almost the whole of the oxide subsides combined with a portion of the soda in a state nearly insoluble.

Precipitated from the alkali by an acid.

When a solution of the white oxide, obtained from eithe of these minerals, has been made, as above, with potash, th whole may be precipitated by the addition of an acid, an will not be redissolved by an excess of sulphuric acid, nitric, of muriatic, succinic, or acetic acids.

Soluble under certain circumstances by some vegetable acids.

But there is a farther agreement in the properties of thes two minerals, which appears above all others, to establis their identity; for though they are both so nearly insolubl by any excess of the mineral acids, yet they are each completely dissolved by oxalic acid, by tartaric acid, or by citric acid; and the solution of each is subject to the sam limitations; for if the precipitate has been dried, it is become intractable, and can scarcely be dissolved again till after a second fusion with potash.

Alkaline solution precipitated by galls.

If to the alkaline solution of either of them there b added infusion of galls, prussiate of potash, or hidrosulphuret of potash, no precipitate occurs; but when a sufficient quantity of acid has been added to neutralize the redundant alkali, the infusion of galls will then occasion a orange precipitate; but prussiate of potash causes no precipitate, nor does the hidrosulphuret precipitate the oxide, although the solution may become turbid from precipitatio of sulphur by a redundant acid.

Precautions necessary in the application of this test.

The characteristic precipitant of columbium is consequently the infusion of galls; but in the employment this test certain precautions are necessary. For as an e

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cess of potash may prevent the appearance of this precipitate, so also may a small excess of oxalic or tartaric acids prevent precipitation, or dissolve a precipitate already formed. A larger excess of citric acid seemed requisite for this purpose, and would also dissolve the gallate of columbium. In each case the precipitate may be made to appear by neutralizing the redundant acid; and for this purpose carbonate of ammonia should be employed: for although pure ammonia has no power of dissolving the oxide alone, yet the gallate seemed to be perfectly redissolved by that alkali.

When infusion of galls is poured upon the white oxide recently precipitated, and still moist, it combines readily, and forms the orange-coloured compound.

Prussiate of potash.

Prussiate of potash occasioned no change in an oxide that had been purified by a second fusion with potash; but it appeared to dissolve a small portion of the oxide, as infusion of galls, poured into the clear liquor, occasioned a cloudy precipitate of an orange colour, though no such precipate took place when the infusion was mixed with the same prussiate alone.

Hidrosulphuret of potash.

Hidrosulphuret of potash being added to the oxide, and heated upon it, impaired the whiteness of its appearance, and seemed to detect the remains of some impurity, which had not yet been removed by other means; but no appearance indicated the formation of a sulphuret of columbium.

The oxides perfectly similar, but the minerals differ in spec. grav.

From a careful repetition of these experiments upon each of the oxides, I see no reason to doubt of their perfect agreement in all their chemical properties; but there is nevertheless a very remarkable difference in the specific gravities of the two minerals from which they are extracted.

very considerably.

The specific gravity of columbite was ascertained by Mr. Hatchett to be 5·918; that of tantalite was found by Mr. Eckeberg to be 7·953; and I have every reason to suppose their results correct, since a small fragment of the former appeared upon trial to be 5·87, while a specimen of tantalite, weighed at the same time, was as much as 7·8. I should, however, observe, that the specific gravities of three other fragments borrowed for this purpose were not so hig that of one being 7·65, of another 7·53, and of a third so lo as 7·15.

This cannot be owing to difference in their proportions.

It is evident, that no variation of mere proportion of tl ingredients can account for an increase of specific gravi from 5·918 to 7·953, which are in the ratio of 3 to 4; f since columbite contains four fifths oxide, if the whole r maining one fifth part in weight of that oxide could be su posed added to the same bulk, without diminution of tl quantities of iron and manganese, the specific gravity wou not then exceed 7·1: and even if a weight equal to one thi of the whole were thus added, without increase of bull still the aggregate would not quite equal the heaviest ta talite in specific gravity; but, on the contrary, the quanti of white oxide in this specimen certainly does not amou to six sevenths, and probably is not more than five sixths the whole mass.

Perhaps from cavities, or mode of aggregation.

The only chemical difference, by which this circumstan could be explained, would be the state of oxidation, whic my experiments cannot appreciate; but it may also arise i part from actual cavities in the mass of columbite, and i part from the state or mode of aggregation.

IV.

Inquiries concerning the Influence, that Light exerts on th Propagation of Sound. By MODESTE PAROLETTE*.

Light an important agent in natural phenomena.

OF all the objects that offer themselves to the contem plation of the chemist and natural philosopher light is th noblest. The action it exerts on all the combinations c matter, its extreme divisibility, the rapidity of its propaga tion, and the part it takes in what constitutes the life of o ganic beings, lead us to consider it as a substance acting th first part in the economy of nature.

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* Journal de Phisique, vol. LXVIII, p. 346.

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It affects other senses besides that of vision.

e magic power this emanation from the heavens exerts ur eyes, in exhibiting to us the spectacle of the unise, cannot be sufficiently admired: but its power is not nfined to the organs of sight; all our senses are subjected to the action of light, and it is from this mode of conceiving its action, that I undertook the experiments, of which I intend to give an account to the academy. The part that I shall treat of at present relates to the effects resulting from the relations that subsist between the rays of light, and the vibrations of sonorous bodies†. The following observations first began to fix my thoughts on this subject.

Its connection with sound.

How first suggested to the author.

In 1803 I lived at Paris. Being accustomed to rise before day, to finish a work on which I had been long employed, I found myself frequently disturbed by the sound of carriages, as my windows looked into one of the most frequented streets in that city. This circumstance, which disturbed me in my studies every morning, led me to remark, that the appearance of daybreak peculiarly affected the propagation of the sound: from dull and deep, which it was before day, it seemed to me to acquire a more sonorous sharpness in the period that succeeded the dissipation of the darkness. The rolling of the wheels seemed to announce the friction of two substances grown more elastic; and my ear on attending to it perceived this difference diminish, in proportion as the sound of wheels was confounded with those excited by the tumult of objects quitting their nocturnal silence.

Confirmed in the opinion, that light assisted its propagation.

Struck with this observation, I attempted to discover, whether any particular causes had deceived my ears. I rose several times before day for this purpose alone, and was every time confirmed in my suspicion, that light must have a peculiar influence on the propagation of sound. This variation however in the manner in which the air gave sounds might be the effect of the agitation of the atmosphere, pro-

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Object of the author.

† I have traced out for myself a series of inquiries, that I conceive must lead to some important discovery. My object is to ascertain the action of light in the various phenomena, that take place in the elastic fluids around us. This object, which on the one hand is connected with the mechanism of our sensations, on the other embraces the results of these first combinations, that escape our apprehension.

duced by the rarefaction the presence of the sun occasion but the situation of my windows, and the usual direction the morning breeze, militated against this argument.

It could not be owing to the wind.

The action of the wind might be supposed capable increasing the propagation of sound, when, blowing in t line from the sonorous body to the ear, it could impart own velocity to the vibrations of the sound ; but it appea to me, that its action must be null, when its motion w perpendicular to that line*. The velocity of sound t which is 1065 feet per second, nearly excludes the influen of the wind, the action of which is slower, and operates o on large masses of the atmosphere†.

Sound has perhaps a peculiar medium.

Sound is propagated by infinitely small vibrations a cording to the theory of Mr. De la Grange‡; and it is pr bable, that this takes place in the particles of a very lig elastic fluid of a peculiar nature, and which should not confounded with the gasses, that compose what we know the atmosphere.

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[*] Hist. of the Royal Acad. of Sciences of Paris, 1738.

[†] Miscel. Phil. Mat. Societ. Turin, tom. 1.

With respect to the action of wind on the propagation of sound, Perrault, formerly member of the Academy of Sciences of Paris, presses himself as follows.

Wind cannot have much effect on sonorous vibrations.

" The invisible particles of bodies, which by their structure and c figuration occasion their essential differences, are themselves co posed of atoms still smaller, and less different in different substan than those particles. Both the atoms and particles are endued w elasticity. When the particles are agitated in such a manner, that elasticity comes to act, on their recoil they strike the particles of the that touch them with the greatest velocity they can impress up them, since it is produced by the spring of their elasticity ; a this velocity is so great, it exceeds that which the air commonly for withdrawing behind the substance that strikes it. Besides, as space in which the spring has acted is extremely small, the air can p this short space forward with greater facility than retire behind the ato The particle of air struck advances a space equal to that, to whi the spring has stretched, pushing that next to it, and so on to the e Hence the sound is propagated with so much velocity ; and other agi tions of the air, as the wind, prevent its propagation but very little, they are too slow with respect to it." Hist. de l'Acad., vol. 1, p. 223.

[‡] Inquiries concerning the Nature and Propagation of Sound, by Le de la Grange.

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An instrument to measure the propagation of sound.

Full as I was of the subject, I thought of means of confirming the reality of the phenomenon by the help of an instrument, which, placing me above all doubt both with regard to the variations of the atmosphere, and as to any illusion that might have deceived my senses, should give me a just measure of the increase of the propagation of sound by the influence of light.

The construction of this apparatus offered its difficulties. The principal was to subject to mechanical proof an object, of which we are accustomed to judge only by our senses. Whatever precision might be desired in such a research, I conceived I might derive some assistance from the experience acquired in practising music, to attain satisfactory results.

An experienced ear, the habit of playing on musical instruments, and the desire of doing right, appeared to me to add to the mechanical means of my apparatus that degree of accuracy, which is requisite in physical demonstrations. The following were my ideas and modes of proceeding.

The vibration of the elastic fluid analogous to those of the particles of the sounding body.

In whatever manner the vibration of a body may be communicated to the elastic fluids surrounding it, it is certain, that the vibration of these fluids is always analogous to that of the particles of the sounding body. A chord stretched and struck with the finger vibrates in a given mode and time. This different regularity in its vibrations it is, that forms the duration of the sound, and the nature of the tone. When a chord vibrates, the tremulation of the circumambient air, which is analogous to the motion of the chord, can communicate itself to a similar chord, if the dimensions and tension are in corresponding proportions. It is a known fact, that if two strings, belonging to two instruments, be in unison, we cannot touch one, without the other's vibrating and emitting a perceptible sound. I thought I might avail myself of this property of elastic fluids, to determine the mode of my experiments.

Apparatus described.

I took two violins of good quality, fitted them up with well chosen Naples strings, and had the pegs made with copper screws, that I might graduate the tones with precision. I placed these two violins horizontally on a plank ten feet long, and eight inches wide. These two instruments having been tuned to the Paris diapason, on that string of one of the violins which is called the second, because it is the second in the neck, I placed a bit of paper, intended to serve as an index in the course of my experiments.

As it was necessary to be able to bring these violins nearer together, or place them farther asunder, and for their movement to be marked on the plank, that served as the base of the apparatus, I arranged them so that one should be fixed, while the other was movable. The fixed was that which had the paper on its second string, and a line corresponding to this string was traced on the plank. The other was movable by means of a very simple contrivance. A little wooden table was held on the plank by means of two grooves; the violin was placed on this so that it could not be shaken; and by means of a screw in the end of the plank I could slide this table with the violin on it backward or forward. An opening in the table parallel with the second string enabled me to mark on the plank the changes of place in my experiments, which I conducted as follows.

Its application. With the forefinger, the other fingers resting on the neck of the instruments, I pressed the second string till it touched the third, and then let it go instantaneously. This fingering, which was done at a place marked on the table, and was always uniform, produced an oscillatory motion, which was heard on the corresponding string of the other violin. The little bit of paper pointed out to me at a distance the vibration of this string, as I separated the two violins till the agitation of the paper became almost null, and at length ceased. This point was that of the limit of the vibration. I marked it on the table that served to support the apparatus, and numbered it 100. The space between the two strings, which were parallel to each other, I then divided into 100 equal parts; and these hundredths were divided into tenths each at the extremity of the table, that I might have thousandth parts in my scale. This first experiment, which was to furnish me a standard of comparison both for the scale of my apparatus, and for the differences in the propagation of sound, was executed on the 14th of May, 1803. Taking the precaution to unite the meteorological observations with the indications of my apparatus, I noted in my pocketbook the degrees of the different instruments I employed at the moment of the experiment. The weather was calm and clear, and the sun shone into my room. The following are the results and meteorological observations of this first trial.

Trial of it.

May 1803.	Thermometer.	Barometer.	Hygrometer.	Apparatus.	Observations.
14	11° 51·8°F.	28·408 30·28 E.	39	100	This experiment, begun at 20′ after noon, was repeated several times. My apparatus always marked the same distance to a few thousandths.

Its scale.

The whole of the scale of my phonometer, which is the name I give my apparatus, answers to 2 met. 14 cent. [7 feet], and consequently each degree is equivalent to about 2 cent. [8 lines].

This done, to proceed methodically I conceived it right to lay down as a fixed principle, that the distance of 7 feet was the limit of the greatest propagation of sound in my apparatus under the influence of light.

Might not the effects of other causes be confounded with those of light?

I was now eager to repeat these experiments in the dark, in order to clear up my doubts of the difference there might be in the velocity and propagation of sound by night and by day. The apparatus I employed promised me results sufficiently conclusive from the length of its scale, and lightness of its motion. It appeared to me, that the least variation must be perceptible, and capable of strict proof, when I could depend on my own attention, and the niceness of my ear. One difficulty only presented itself to my mind, which arose from the influence, that variations in the temperature, weight, and humidity of the atmosphere might have on the phonometrical changes. I was aware, that the propagation of sound must be according to the nature and density of the elastic fluids it traverses; and I was apprehensive of being led into errour by a cause foreign to that which was the object of my inquiry: but I also knew, from the experiments made in 1738 by Maraldi, De la Caille, and Cassini, and from those of Bianconi in 1740, that the thickest fog had been found to have scarcely any effect on the velocity of sound. To proceed more satisfactorily therefore, I determined first to examine by means of my apparatus and meteorological observations, the effect of changes in the atmosphere on the velocity and propagation of sound.

Trials of the effect of changes in the atmosphere.

These trials I made on the 9th, 12th, and 15th of September, the same year, each time about noon, and with striking differences in the state of the sky and atmosphere. The following table exhibits the results of these.

Sept, 1803	Therm. Fahr.	Barometer.	Hygrometer.	Phonometer.	Observations.
9	62·96°	30·37	58°	99·9	Half an hour after noon. Sky clear.
12	53·6	30·32	51·5	99·7	45′ after noon. Sky covered with thick clouds.
15	59·72	30·12	51	99·8	40′ after 1. Cloudy and threatening rain.

Trifling, if any.

The small differences in the results of these experiments, the greatest of which does not exceed three thousandth parts, appeared to me not ascribable to changes in the atmosphere. In my opinion they are the consequence of the imperfection of my method, which is incapable of mathematical precision. Not being able at the moment to procure more certain data for this discussion, I was inclined to think, that the common changes of the atmosphere could not effect such a change in the nature and arrangement of the particles of the elastic fluid, as to restrain or accelerate the agitation occasioned by sonorous bodies.

Experiments in the dark.

Nothing remained but to ascertain by strict trials the decrease of the propagation of sound in the dark, which should give the solution of my problem. I began my experiments the 20th of September following, and I chose a night and an hour best adapted by the darkness of the weather to show the effect of the absence of light.

The place of the experiment was lighted by a watch lamp of a particular construction, which afforded light enough to perceive the motion of the paper on the string, without the rays being able to diffuse themselves through the chamber. The following were the results.

Sept 1803	Thermometer.	Barometer.	Hygrometer.	Phonometer.	Observations.
20	53·24°	29·79	65	98·1	11 o'clock at night. The sky covered with clouds.

I was satisfied with this trial. Eager to make it public, I spoke of it to some friends, who encouraged me to repeat these trials, and to attend particularly to this subject, which might prove highly interesting to the progress of science. Private affairs prevented me from prosecuting it at that time, and I did not resume it till about ten months after. The following are the results of the experiments then made, which tended only to confirm the preceding.

Table of comparative experiments.

July 1804	Thermometer.	Barometer.	Hygrometer.	Phonometer.	Observations.
3	66·2	28·89	73·5	100	About noon. The sky free from clouds.
5	55	28·90	66·5	97·5	Quarter after 11 at night. The sky partly covered with clouds.
11	57	29·86	68	99·8	1 o'clock afternoon. The weather fine.
14	62·24	29·87	51·5	99·4	Quarter after 1 P. M. Raining
18	74·66	28·90	62	98·4	Just after midnight. The sky cloudy.

General conclusion.

Such have been the constant results of my researches. The mean term of the degrees of propagation of sound without light in three different trials was found to be 0·98.

The difference between the propagation of sound that took place during the night from that in the day comes out two degrees of my scale, answering to about 16 lines. Whenever I made any experiments on this subject I always acted with the greatest caution, taking care to guard against the slightest inaccuracy. The subject has always appeared to me difficult and delicate; and without venturing to assert too much, I confine myself to an account of my thoughts and inquiries, happy to have removed a doubt, that concerns one of the most important branches of our knowledge.

Facts should be compared.

After having called the attention of natural philosophers to a discussion, which may serve to render the nature of luminous bodies better known, as well as to elucidate the nature of sounds, and to discover the mutual action of the imperceptible substances that surround us, it seems to me not improper, to compare together the facts, that appear to relate to this subject, and which may give rise to discussions calculated to explain the phenomena.

Night supposed more favourable to the propagation of sound, but merely from the absence of noise.

Hitherto night has been considered as more favourable than day to the propagation of sound. That this is the case with respect to our ears cannot be doubted: but this argues nothing against my opinion. We hear farther by night on account of the silence; and this silence contributes to it, while, according to the celebrated Euler, the noise of a wind favourable to the propagation of a sound may prevent the sound from being heard*. I have reason to think, that our ear has more aptitude for hearing sounds by day than by night, and this from the stimulant action, that light exerts on the nervous system: but this will not account for the phenomenon

The ear hears best by day:

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* History of the Academy of Sciences at Paris, 1738.

Distinction between sound and noise.

Sound and noise, which are the same thing with respect to the object of my inquiries, exhibit essential differences when considered with regard to our ears. By sound is to be understood that peculiar resonance, which proceeds from a sonorous body, and the tone of which we know. Noise I conceive to be an assemblage of several sounds.

When any sound predominates our ears can distinguish it from other sounds in harmonic proportion with it. In noise the harmonic sounds are confounded and lost. The celebrated Condillac, speaking of the nature of sounds in his treatise on Sensations, has distinguished the two by the definition of appreciable sounds and inappreciable sounds.

but this will not explain the effect on the apparatus.

phenomenon exhibited by the paper, the motion of which depended on the tremulation of the air of the room. Is the atmospheric air more dense on the appearance of light than in darkness? Is this greater density of the air, or of the elastic fluid that is subservient to the propagation of sound, the effect of aeriform substances kept in this state through the medium of light? On this hypothesis we must suppose, that a greater density may take place without increasing the weight of the air; and we might ascribe the increase in the propagation of sound to a greater elasticity in the fluids diffused through the atmosphere. This would confirm the opinion of Dr. Priestley, who said, that sound was propagated in different gasses in the ratio of their density. But the rays of the solar light are inseparable from the calorific rays: their presence therefore, by raising the temperature, muſt produce a dilatation in the circumambient air, that seems to exclude the hypothesis of condensation. Besides, it is proved by the experiments of Mr. Perolle, inserted in the 3d vol. of the Memoirs of the Royal Academy of Sciences of Turin, that the propagation of sound is affected, not merely by the density of the different gasses it traverses, but by their nature.

Priestley's opinion.

Perolle found, that sound was not in the ratio of the density of the medium.

Mr. Perolle having placed an alarum watch in a jar which he filled successively with different gasses, retired gradually from the apparatus, stopping at the point where the sound was no longer audible to him. Proceeding in this manner he found, that the weight of a cubic foot of each of the gasses tried being,

Carbonic acid gas	1080,
Oxigen gas	765,
Atmospheric air	720,
Nitrous gas	698,
Hidrogen gas	72,

the propagation of sound followed an order not always analogous to that of the density. It was as follows.

In

	feet.	feet Eng.*
In atmospheric air	59	98·88
carbonic acid gas	48·4	81·11
oxigen gas	66·5	111·45
nitrogen gas	the same	
hidrogen gas	13	21·79

Oxigen best adapted to its transmission.

The results of this experiment seem to prove, that of the different gaseous substances oxigen is best adapted for transmitting the vibrations of sonorous bodies; and the equality of effect obtained with nitrous gas, which contains 0·56 of oxigen, gives reason to think, that the increase of the propagation of sound has a determinate measure, and that a given quantity of oxigen gas diffused in the atmosphere is sufficient, to carry it to its maximum†.

Is this the cause of the effect of day-light?

This reasoning appears to me the more satisfactory, as it may connect the results obtained by Mr. Perolle with those of my experiments. It is certain, that during the day, and under the influence of the light, the atmospheric air is more saturated with oxigen than during the night; but it remains to be proved whether this surplus of oxigen, which from the nicest eudiometrical researches cannot exceed a few hundredths, can be capable of producing such a remarkable change.

Or rather, is it not light that acts in oxigen and nitrous gas?

On the other hand, when it is proved, that the density of gasses is not the only reason of the acceleration of the progress of sound; and my experiments appear to demonstrate a certain influence in light; may we not consider the latter as the true cause of the increased propagation of sound in oxigen and nitrous gas; since we know, that oxigen has a great capacity for light, and that nitrous gas cannot be formed without the concurrence of this substance? Whatever may be the opinion of natural philosophers on this subject, it is certain, that the hypothesis reconciles my experiments with those of Mr. Perolle; and that together they may lead to very important researches.

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* I suppose the feet in the preceding column to be those of Turin. C.

Light has considerable effect in chemical action.

† The illustrious Dr Bonvoisin, who, in his Elements of Chemistry, has taken every opportunity of pointing out to his pupils the influence, that light exerts on a number of chemical actions, particularly notices the necessity of the concurrence of light in all the combinations of nitrogen with oxigen. See vol. 1, art. Acide, nitrique.

The action of light not merely mechanical.

Light has a velocity 900000 times as rapid as that of sound. Whether it emanate from the Sun, and reach to our Earth, or act by means of vibrations agitating the particles of a fluid of a peculiar nature, the particles of this fluid must be extremely light, elastic, and active. Nor does it appear to me unreasonable, to ascribe to the mechanical action of these particles set in motion by the sun the effects its presence occasions in the vibrations that proceed from sonorous bodies. The more deeply we investigate the theory of light, the more we must perceive, that the powers by which the universe is moved reside in the imperceptible particles of bodies; and that the grand results of nature are but an assemblage of an order of actions, that take place in its infinitely small parts: consequently we cannot institute a series of experiments more interesting, than those that tend to develope the properties of light. On the least success of such inquiries we flatter ourselves with the presage of some important discovery: our organs of sense are so immediately connected with the fluid that enlightens us, that the notion of having acquired an idea of the mode of action of this fluid presents itself to our minds as the hope of a striking advance in the knowledge of what composes the organic mechanism of our life, and of that of beings which closely follow the rank assigned to the human species.

The greatest natural phenomena owing to the affections of the minutest particles.

V.

On the Chemical Action of simple Galvanico-electric Chains formed of Metallic Solutions, Water, or Acid, and a Metal; and on the Disoxidation of Metallic Oxides effected by these Means: by Mr. BUCHOLZ*.

Galvanic electricity.

THE electricity produced by the pile of Volta and galvanic chains has presented philosophers with the most surprising results, particularly with respect to chemistry. Who can forget the decomposition of water effected in a peculiar manner, the oxidations, the disoxidations, the hidrogenations first accurately observed by Ritter, as well as the decomposition of several acids, salts, &c.? We might have hoped, that chemists would have paid more attention to electricity, and that they would have endeavoured to derive more advantage from it; but this has not been done in the degree, which the importance of the subject appeared to demand. Ritter too has the merit of having pointed out to chemists the influence of the electric matter on chemical phenomena: and though the opinions he started in his work on the Electric System of Substances may not be altogether accurate as to the effect of the electric matter in chemical actions, yet we cannot wholly deny its influence; particularly as Sylvester showed in 1806, that the precipitation of one metal by another was nothing but a galvanico-electrical process*; which confirmed the opinion expressed by Ritter in 1800, though this opinion can be admitted only in the case of metallic vegetations, and not for every sort of reduction in the humid way; because a chain of two metals and a fluid is not always formed, when one metal is reduced by another, and yet the result does not remain a moment doubtful.

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* Annales de Chimie, June, 1808. p. 266. Translated from Gehlen's Journal, No. 17.

Its influence in chemical phenomena.

Precipitations of metals.

The influence of electricity in chemical processes, with respect to oxidation and disoxidation, is still more evident, when chains are formed with two fluids and one metal, as Ritter observed in 1800; and the reality of which he demonstrated on the occasion of a remark I made in 1804, that tin precipitated muriate of tin in a metallic form, when water was poured into a solution of muriate of tin, and a slip of this metal was immersed in the solution and the water at the same time. The utility and necessity of studying the influence of electricity could never be shown in a more determinate manner, than in the case in which Ritter has explained the most enigmatical phenomena in a natural and easy manner. I have lately been more sensible of the justice of this explanation, as I have confirmed it by

In galvanic chains of one metal and two fluids the metal displaces its own oxide.

Tin.

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[*] See Journal, vol. XIV, p. 94.

amining, whether other metals were capable of forming ilar chains with their solutions and water. I communi-ate these experiments, because I know of no one except itter, who has attended to these chains, and they have peared to me interesting with respect to the particular cumstances accompanying them, though they are all unded on the same system.

Experiments with solutions of copper, another fluid, and copper.

Experiment with crystallized green muriate of copper.

1. Into a cylindrical glass I poured a solution of half an ounce of crystallized green muriate of copper in two ounces of water, and added with the greatest caution three ounces of distilled water, so that the liquors remained separate, one above the other. I then immersed in the two liquors a slip of polished copper, half an inch broad and six inches long, which rested on the bottom of the glass. At the expiration of two hours no action appeared to have taken place, except that the slip of copper was covered with a white crust, that continued increasing for twelve hours. On examining this substance, it was found to be white muriate of copper, formed by the oxide of the green muriate sharing its oxigen with the metallic copper immersed in the liquor.

Exp. with white muriate of copper.

2. As the preceding experiment had shown, that the green muriate of copper could not form a chain capable of reducing completely the oxide of copper held in solution, I was desirous of seeing whether the white muriate would comport itself differently. Accordingly I boiled a drachm of this salt with three ounces of water for a quarter of an hour, filtered it when cold, and placed the solution in contact with two ounces of distilled water and a slip of polished copper as in the preceding experiment. Having obtained no precipitate of copper at the expiration of a few hours, or even in some days, I ascribe this nullity of effect to the little difference between the specific gravity of the two fluids; for it is well known, that water dissolves very little white muriate of copper; whence the two fluids will unite immediately and destroy the chain.

3. Half

Exp. with nitrate of copper.

3. Half an ounce of copper was dissolved in nitric ac taking care that there should be no greater excess of a than was absolutely necessary for the solution of the s The solution being evaporated to two ounces, the chain formed by adding three ounces of distilled water above and immersing in the two fluids the slip of polished c per, taking care to keep it a few lines from the bottom the glass by fastening it to a bit of cork. The two flu appeared at first quite separate from each other: but a f minutes after the chain was formed, a narrow but v bright transverse line appeared on the copper at the po of their separation; above this point the copper was mu darker; and at the end immersed in the fluid, small a almost imperceptible filaments were deposited, which g dually increased, but ultimately disappeared. The brig line increased greatly, and the part that was in the wa grew darker, till it became a brown black. At the end 72 hours, when the two liquids appeared to be complet mixed, and the activity of the chain destroyed, I took the slip of copper, and found it in the following st Near the centre there was a bright place about a quar of an inch broad, where neither oxidation nor precipitat of the copper was perceivable. Above this line was a slig coat of brown black oxide of copper; and below a red p verulent stratum of copper, which gradually thickened ward the end of the slip, and assumed a striated appe ance. This stratum, viewed with a lens, appeared to formed of a collection of little grains, which acquired metallic lustre on being rubbed. The result of this ex riment showed evidently, that, under suitable circu stances, a very active electro-galvanic chain might be form in this manner, by means of which copper might be p cipitated in the metallic state by means of copper itself.

Nitrate of copper and water acidulated with nitric acid.

4. To learn what would be the reductive power of su a chain, if the water employed were acidulated with $\frac{1}{10}$ nitric acid, I proceeded nearly as in the foregoing expe ment. The result was much the same, only the slip copper was more speedily oxided in the acid liquor, so bubbles of gas escaped, and the copper was deposited i thicker stratum, and with a lustre almost metallic, at t

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bottom of the slip. The experiment terminated in much less time.

Nitrate of copper with a little excess of metal.

5. To vary the third experiment, I made a solution of copper in nitric acid, so as to leave some metallic copper in the saturated solution; and then formed a chain. At the expiration of a few minutes the usual phenomena presented themselves, except that at the point of contact of the two liquors a slight turbidness appeared, occasioned by the precipitation of the nitrate of copper at a minimum in the form of small flocks of a greenish white. At the commencement some flocks were deposited on the edges of the slip of copper, but they afterward disappeared. As, when water had been added, the liquor was rendered absolutely turbid by the separation of the oxide of copper, which was present in excess, I took out the slip of copper, and was agreeably surprised by an unexpected result: as far as the slip had been immersed in the cupreous solution, it was covered with another slip of copper extremely thin, on which were several little knobs of the size and shape of pin's heads, which looked metallic, smooth, and as if they had been melted, and viewed with a lens had one or two little openings on the side. It appeared to me too, that the flocks observed on the edges of the slip had been converted into similar little knobs, for the edges were covered with them in different places. Whence could arise this interesting phenomenon of the separation of the copper in the form of little globules?

Nitrate of copper with excess of acid.

6. This experiment was again varied by allowing the solution of copper to contain a slight excess of acid. The result was the same as in experiment 3, except that the copper was longer in being precipitated; this not taking place till the excess of acid appeared to be deadened, which was effected with the extrication of a great many bubbles of gas; and the separation took place only at the bottom of the slip, and in the form of little grains.

Reduction of copper most complete with a saturated & perfectly neutral solution,

Many other experiments, infinitely varied in order to obtain more extensive cupreous vegetations, taught me, that the reduction of copper by means of copper was more complete in proportion as the solution was more concentrated, the point of saturation more exact, and the slip of copper broad, and accelerated by slightly acidulating the water.

broad, thick, and polished; and that when a little ı acid was added to the water the result of the operatior accelerated by the oxidation of the metal being effe more copiously in the superior fluid: but that a too ɉ excess of acid was unfavourable to its success, the pr going on with too much commotion, and producing speedy a mixture of the liquors, as well as too great a cific gravity of the upper, which destroys the action of chain.

Experiments with the solution of nitrate of silver, water, silver.

Exp. with nitrate of silver.

7. Into a small narrow glass I put a solution of drachms of nitrate of silver in half an ounce of water, poured on it six drachms of distilled water, acidulated ı half a drachm of pure nitric acid. The chain was c pleted by a little piece of solid silver, narrow and end in a point, which was fixed in a bit of cork. The ac was almost instantaneous. A point of 0 was formed, ı marked by a very bright transverse line, that remained tact. Immediately below this point metallic silver separa in shining grains resting one against another in a horizoı line, which continued to be deposited the whole length the slip, which was half an inch. These grains increa for twelve hours below the point of 0. The slip of sil became gray, and was gradually covered with black oxi At the expiration of this time the action of the silver w no longer perceptible, and the slightest touch occasioned to drop asunder, as it was corroded where the acid liqu was in contact with the air. During the whole process th were but few bubbles extricated in the upper liquor. T slip of silver covered with little shining knobs exhibited pleasing sight, when viewed either with a lens or the nak eye. Professor Bernhardi examined them with a micr scope, which magnified 200000 times, and then di tinctly perceived, that they were cubes truncated at tl edges.

The same with a larger quantity.

8. I repeated the preceding experiment with a view to e fect a more considerable separation of silver. According

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ains of silver were deposited, so small as to be scarcely guishable by the eye. In 72 hours these points had ed the size of pin's heads in seven or eight places. pper part of the slip was turned of a deeper gray, but liquors being perfectly mixed, there were no hopes aining a more complete result.

Activity in proportion to the oxidation.

s experiment affords fresh proof, that the activity of bears a certain ratio to that of the oxidation.

Nitrate of silver and water acidulated with nitric acid.

The liquors of the preceding experiment were evapo- to six ounces, and after being returned into the same five ounces of water acidulated with one of pure acid of the specific gravity of 1·25 were added with a deal of caution. The chain was completed with the silver that has been mentioned. The action began diately. A point of 0 was apparent a quarter of an broad, where the slip of silver remained unaffected. this point was formed black oxide; and beneath, ghout the whole breadth of the slip, a line of knobs ining silver, exactly as in experiment 7, but in a hat larger proportion. These little grains increased hours, as well as the black oxide. The silver sepa- had the same form as in experiment 7, except that agglutinations of silver at the point of the slip were though they became shining on being pressed upon hard substance. On taking out the slip at the ex- n of 36 hours, it divided into two parts. It was par- ly corroded where it had been in contact with the at the upper part, at which place it was completely

Experiments with solutions of lead, water, acidulated u and lead.

Exp. with nitrate of lead.

10. A chain was formed with four ounces of a soluti nitrate of lead made without heat, four ounces of w and a slip of lead an inch broad, six inches long, and an inch thick, fixed in a piece of cork laid across th of the glass. Immediately a point of 0 was perceived, w the liquors were in contact, and this continued gro broader and more perceptible during the course of an Above this point the brightness of the lead continue minishing; and below small shining metallic laminæ deposited, which increased for 24 hours. At the en this time the liquors had mixed, and destroyed the c The metallic laminæ, which were pure lead, had no lar figure.

Nitrate of lead, & water acidulated with nitric acid.

11. To learn what would be the action of a similar c if acidulous water were employed, I repeated the ex ment with the addition of two drachms of acid to the w Soon after a brighter place was observed where the tw quors were in contact; above this gas was extricated, white oxide was formed; while below the slip of lead dull. After the expiration of twenty-four hours oxide formed at this place also, and gas was evolved. At period the liquors had mingled together, without any s ration of metallic lead. According to all appearance t had been no very active chain, because the liquors m too soon. To verify this supposition, I made the follo experiment.

Warm solution of nitrate of lead and acidulated water.

12. Four ounces of boiling distilled water were satur with nitrate of lead, and put milk-warm into a cylind glass. Four ounces of water acidulated with an ounc nitric acid at 1·25 being added upon the solution, the c was completed with a slip of polished lead. A point formed, that appeared more shining than in the prece or following experiments. The part of the lead in acidulous water was covered with a great many bubble gas, and became gray. On the edges of the slip that v immersed in the solution were deposited filaments, wl at the expiration of some hours increased, so as to f

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little eminences of scales of lead a third of a line long; and at the same time crystals were deposited, which were in part attached to the slip of lead, and in about twelve hours had covered the slip, and formed a fine groupe interspersed with scales of lead. It was only near the point of 0, where the solution was weakened by the supernatant liquor, that no crystals were deposited with the scales of lead. The upper part of the slip of lead was covered with gray oxide.

The specific gravity of the fluids should differ.

The result of this experiment showed I had not conjectured without reason, that no lead was separated in experiment 11 because the specific gravity of the liquors differed too little, which occasioned the electric fluid to be badly conducted, and the liquors to mix too soon and destroy the chain.

Exp. with acetate of lead.

13. A solution of an ounce of acetate of lead in three ounces of distilled water, forming a chain with five ounces of water and a slip of lead, had experienced scarcely any change at the end of 24 hours, except that the lead appeared a little duller toward the bottom, and a little brighter toward the top.

Acetate of lead and distilled vinegar.

14. I evaporated the superfluous water from the solution of lead, and employed as the upper liquor distilled vinegar. A few minutes after the chain was completed the point of 0 was perceptible, above which the slip of lead became of a whitish gray, and below it several points of a blackish gray were formed. The latter, after the expiration of 36 hours, had increased so as to cover almost the whole of the lead immersed in the liquor. These points, examined with more care, exhibited the form of little knobs. They were friable, which led me at first to take them for oxidule of lead; but having rubbed them with a hard substance they acquired the metallic brilliancy, which showed, that they were metal.

Experiments with muriate of zinc, water, acidulated water, and zinc.

Muriate of zinc,

15. I formed a chain with half an ounce of zinc dissolved in muriatic acid and diluted so as to form four ounces of solution, five ounces of water, and a slip of zinc. Soon

after the chain was completed, a blackish gray substance was deposited on the point of the zinc immersed in the solution, and this continued increasing for 48 hours, while a grayish coat of oxide of zinc was depositing on the part in the water. The ramifications of zinc, that were on the lower part, had the aspect of an oxide; but on rubbing them with a hard substance they acquired the brilliancy of zinc.

Muriate of zinc and acidulated water.

16. This experiment was repeated, only acidulating the water with two drachms of muriatic acid. The result was nearly the same, except that less zinc separated, because the chain did not remain so long active, the liquors having mingled too soon.

Experiments with solutions of iron, water, acidulated water, and iron.

Green sulphate of iron.

17 and 18. An ounce of green sulphate of iron was dissolved in three ounces of water, and a chain was formed with four ounces of water, and a bar of very soft iron. I observed no difference between the action that took place on the part of the bar in contact with the water, and that in contact with the solution of iron, but in both the iron was coated with a stratum of black oxide of iron, and some yellow flocks resembling oxide separated from the solution. The same effect took place when the water was acidulated with a drachm of concentrated sulphuric acid. In neither of these experiments was any electric polarity observed, or point of 0.

Muriate of tin.

19. Four ounces of fuming muriatic acid were gradually saturated with pure iron filings. With this solution, four ounces of water, and a bar of soft iron, a chain was formed. It did not appear to have more effect than those of the two preceding experiments, for no difference was observable in the state of the iron in the upper and under liquors. In each the bar of iron was covered with black oxide, and flocks of brown yellow oxide were gradually deposited.

Muriate of iron and acidulated water.

20. On varying this experiment by adding two drachms of fuming muriatic acid to the four ounces of water, a solution of the iron in the acidulated water took place, with the

evolution of gas. The iron in contact with the solution was covered with a coat, that had sometimes the colour of indigo or of copper, and acquired the brightness of iron by rubbing; which appeared to indicate a separation of iron in the metallic state, and in a pulverulent form, though I am not inclined to adopt this opinion. The too speedy mixture of the two liquors having destroyed the chain, I could not procure myself enough of this black powder to make an accurate examination of it, and to determine what it was with more certainty.

Conclusion.

General deductions.

From all the experiments I have described it appears to me we may infer, that almost all metals are capable of forming a chain with their own solutions and water, the electric action of which precipitates the metal in the metallic state; and that an evident anomaly appears to take place, as if the metal dissolved were precipitated by the pure metal, which would seem contradictory to the received laws of affinity between a metal and oxigen. It has always appeared to me, that the following conditions are necessary to our complete success.

The acid must be one that acts but little on the same metal:

1. The metal must form a solution with some acid, which is altered very slowly or not at all by the pure metal; and which consequently contains no excess of acid or of oxide; in order that the electric current formed by the chain may act without being checked on the metallic oxide in the solution.

the solution must be strong:

2. The solution must be sufficiently concentrated, not to mix easily and readily with the supernatant liquor, and annihilate the chain.

and the metal must be oxidable by water.

3. The metal, that is to form the chain, should be oxidable by water, and thus determine the electric current so as not to occasion a mixture of the two liquors, and thus destroy the chain too quickly.

Other metals.

It is to be presumed, that gold, platina, bismuth, manganese, tungsten, and other metals, would likewise form electro-galvanic chains, and exhibit the apparent anomaly of a piece of metal precipitating the oxide of the same metal from its solution, which I will examine when I have leisure, unless I am prevented by others.

Addition to the preceding paper.

The results confirmed.

I have farther to remark, that the results of my experiments on the precipitation of copper confirm an observation announced by Mr. Buenger; but for more certainty I made the following experiment.

Farther exp. with sulphate of copper.

21. I formed a chain by putting into a cylindrical vessel a solution of two ounces of sulphate of copper in six ounces of boiling water, on this I poured six ounces of water, and I immersed in the two liquids a slip of copper. At the expiration of 24 hours no change appeared on the slip of copper, either in the cupreous solution, or in the water. Some sulphate of copper however was separated, and the two liquors were well mixed.

Sulphate of copper and acidulated water.

22. I repeated the former experiment exactly, except that I added an eighth of sulphuric acid to the water employed. The result was, as soon as the chain was formed an oxidation and disoxidation of the metal took place, and a point of 0, ¼ of a line broad, formed on the slip of copper. At the expiration of 48 hours, as the action no longer increased at the two poles, I took out the slip of copper, and found, that the whole of the part immersed in the solution was covered with a solid but thin coat of copper, which viewed in front had the colour of pure copper, but dull and without brightness; but viewed obliquely had a velvety appearance, and a paler colour. Seen through a lens this crust appeared crystalline, and of a metallic brightness. The slightest rubbing gave it the polish of the purest copper. Between the part covered with this coppery incrustation and the oxided part there was a stripe a third of a line broad, where the slip of copper remained unaffected; and above this, for the space of two inches, the copper was covered with black brown oxide.

VI.

VI.

Extract of a Letter on Potassated Iron from Mr. HASSENFRATZ, *Engineer in Chief of Mines, and Professor of Mineralogy at the Practical School of Mines, to Mr.* GILLET-LAUMONT, *Correspondent of the Institute, and Member of the Council of Mines**.

Alkalis decomposed by iron.

Iron smelted with charcoal.

YOU are acquainted with the beautiful experiments, in which Gay-Lussac and Thenard have decomposed potash and soda by means of iron, and obtained metals that combine readily with iron, producing alloys, from which nitrate of potash is obtained when they are treated with nitric acid. You know too, that crude iron is pretty generally obtained in France by fusion with wood charcoal, which contains more or less potash; and that this is afterward refined with the same combustible to produce malleable iron; whence it is highly probable, that the potash contained in the charcoal is reduced, and afterward combines with the iron in the processes it undergoes.

Proportions of potash in charcoal.

Oak charcoal yields about eight parts of saline matter in a thousand, beech five, elm twenty, poplar six, fir two, &c. A mean of the charcoal commonly employed will produce seven parts of salt to a thousand of fuel; and if 500 parts of charcoal be used on a medium to 100 of iron, it follows, that two or three per cent of the new metal may combine with the iron formed.

Iron injured by small quantities of alloy.

The small quantity of the new metal that can combine with iron in the different processes it undergoes might remove the apprehensions of those metallurgists, who are inclined to ascribe injurious effects to it, that quantities as inconsiderable of phosphorus, sulphur, copper, &c., will render iron, the first coldshort, the other two redshort.

Trials should be made on a large scale,

and these seem to show, that too much potash injures iron.

From the small combinations of potassium and iron hitherto made, it is difficult to form a judgment of the influence of this new metal on the goodness of iron, because the quantities obtained were too small to be forged: but notwithstanding the smallness of the buttons of potassated iron that have been fused, some men of science have thought, that the compound was brittle. When coldshort iron is purified at Zinswiller on the Lower Rhine with lime and potash, iron of good quality is obtained, if the quantity of the substances added in the refining be suitable; but the iron becomes redshort, if they be employed in too large proportion, whence it follows, that lime and potash are capable of rendering iron brittle.

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* Journal des Mines, vol. XXIII, p 275.

Does it render it redshort?

The opinion of some scientific men, who presume that the action of potash must render iron brittle; and the experiment in the large way at Zinswiller, which would induce a belief, that potash occasions it to be redshort; would lead metallurgists to ascertain by direct and positive experiments the influence of potash in its combination with iron.

The author writing on the art of iron founding.

Being at this moment engaged by order of his excellency the minister of the home department in writing on the art of treating and preparing iron, it was natural for me to attend to this question. Accordingly I requested the two respectable chemists, to whom we are indebted for the discovery of the reduction of potash by means of iron, to favour me with one of the gunbarrels, in which they had repeated their experiments a great number of times at the Polytechnic School. They had the goodness to choose me one, the inside of which was interiorly filled with potassated iron, and which the combination of potash had rendered so fusible, that it had in part melted during their experiments.

A gunbarrel that had been repeatedly used for reducing potash, examined.

The iron of this gunbarrel, penetrated with potash, was assayed in presence of one of the pupils and myself by the locksmith Rosa, who is commonly employed by the Council of Mines for the various trials of iron and steel.

Heated and flattened.

The gunbarrel was heated and hammered flat for the length of four inches on each side of the part fused by the combined action of the potash and heat.

Piece of alloy broken off.

Its properties.

In flattening it a portion of superpotassated iron was separated, which we picked up. This little piece, weighing 4 gr. [62 grs.], was coldshort. In its fracture it exhibited a mixture of a brown substance and a white. The latter had the metallic lustre, and was pretty easily hammered when separated from the brown substance. After 24 hours exposure to the air the small piece was covered with moisture, which it had attracted from the atmospheric air.

The barrel forged and welded, bent and twisted hot

The barrel after being flattened and heated was forged and welded very perfectly. It extended under the hammer as easily as coldshort iron. It was bent and twisted backward and forward several times, without the least crack appearing.

A small

and cold. Broke.

A small bar four lines thick was bent backward and forward cold, but it broke after repeated bending. The outer surface of the iron was a dull platina white. The fracture exhibited a white rim, fine grained, and strongly compressed, from 8 to 15 tenths of a line broad; and this rim enclosed a brown nucleus from 2 lines to 2·75 broad. The nucleus, completely separate from the rim, and having but a slight adhesion to it, was composed of white and brown grains weakly united. It was formed of the interior surface of the barrel, which had been most exposed to the action of the potash. The outer iron was soft, and easily filed and flatted cold.

Tempered.

Another bar forged thin was tempered, after being heated to a cherry red. Its surface was hard when filed, and the metal retained its malleability. It did not break, till it had been bent several times. Its fracture appeared similar to that of the iron which had not been tempered.

The iron combined with potassium in two proportions:

From all these trials we may conclude, that the iron appears to have combined with the potash in the gunbarrel in two different proportions; one a minimum, which gives iron a white colour resembling that of platina; the other a maximum, which gives it a brown colour mingled with white specks:

a small proportion improved,

That the iron potassated at a maximum is easily wrought hot or cold, that it becomes more malleable by this combination, and that it can acquire hardness by tempering without becoming brittle like steel; consequently it is highly probable, that the small quantity of potassium, which combines with iron when it is smelted with charcoal, can only contribute to improve its quality:

a large injured it:

That iron potassated at a maximum acquires a brown hue more or less deep, mingled with larger or smaller white specks; and that in this state the particles have less cohesion, so that the iron becomes coldshort, and very probably redshort.

but one trial not sufficient.

A single experiment on the combination of potash with iron may give some idea of the new properties the metal acquires during and after it is wrought; but it would be too hasty to conclude absolutely respecting the qualities of potassated iron from a single trial. We must wait therefore till farther experiments shall confirm the whole of the results we obtained, refute some of them, or add new ones, when we may decide with more certainty.

VII.

VII.

Analysis of the Aerolite that fell at Stannern in Moravia, the 22d of May, 1808. *By Mr.* VAUQUELIN*.

Meteoric stone of Stannern.

THIS aerolite in its external appearance resembles other productions of the kind. It is covered externally with a brown, vitreous coating or glaze. Interiorly it exhibits a gray substance, interspersed with black dots; and in several places there are shining laminæ, that appear to be pyrites, for they are not attracted by the magnet; nor does the stone itself act on the magnetic needle. This substance is not homogeneous, for pretty considerable nodules, much blacker than the rest of the stone are visible in it to the naked eye.

Contained the same principles as basaltes.

Mr. Klaproth, having analysed a specimen of this mineral which was sent him in powder, and found it to contain the same constituent principles as basaltes, and nearly in the same proportions, was desirous of a description of it in substance, in order to compare it with basaltes. Accordingly Count Unin, who has a very fine specimen of it, has given the following description.

Its appearance.

The surface of the meteorolite of Stannern is fused; and perfectly black. This characteristic, which is peculiar to meteorolites, distinguishes them from other stones. The colour of its substance is a light ashen gray, which is not altered by scraping. Interiorly are perceived more compact grains, and of a darker colour than the rest of the mass. It contains likewise grains of sulphuret of iron, but in small quantity.

Its properties.

This stone is soft, friable between the fingers, not scratching glass, and giving no sparks with steel. Its specific gravity is 3·19. It fuses with difficulty before the blowpipe into an opake glass, which is attracted by the magnet.

Contains a

From the specimen I possess, which I picked up on the spot, the meteorolite of Stannern differs from others only in containing a smaller quantity of metallic substances. proportion of metal,

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* Annales de Chimie, vol. LXX. p. 321.

Mr. Klaproth, from its chemical analysis, suspects a similitude between this stone and basaltes, but it is very certain, that the two substances differ eſſentially in their fracture, hardness, and appearance when scraped. but does not resemble basaltes.

The meteorolite that fell near Eggenfield in Bavaria, in December, 1803, is that which in its external characters comes nearest to the basaltic tufa in the neighbourhood of Klosterlaach. Mr. Chladni possesses a specimen of it, which is very remarkable from containing olivine, or the granular peridot of Haüy, disseminated in it. Stone of Eggenfield more like basaltes. Contains olivine.

Mr. Moser, a chemist of Vienna, found in 100 parts of this aerolite the following substances. Component parts of the stone of Stannern according to Moser.

Silex	46·25
Alumine	7·62
Black oxide of iron	27
Oxide of manganese	0·75
Lime	12·12
Magnesia	2·50
A trace of chrome, and of muriates	
Sulphur, water, and loss	3·76
	100

These results, so different from what have hitherto been found in productions of this kind, having led some persons to suspect, that there may have been errours in this analysis; and conceiving at any rate, that it deserved a repetition; requested me to undertake it, which accordingly I did. Analysis of it.

Treated by muriatic acid, this stone gave out a very small quantity of sulphuretted hidrogen. Treated with muriatic acid.

If it be subjected to a strong red heat, its colour changes to a pale red, but its weight is not diminished; no doubt because the oxidation of the iron makes up the loss of weight. Heated.

Treated with caustic potash we obtain a fused substance, which has a green hue. This colour is more distinct on diluting the substance with water, which produces a deep green solution. After the solution is filtered, on being exposed Treated with caustic potash.

posed to the air a slight flocculent precipitate of oxide of manganese falls down. On filtering afresh the solution has a fine yellow colour, which would be supposed to be owing to chrome. It was then saturated with nitric acid, and evaporated to dryness. During this operation it assumed the appearance of a jelly, which indicated the presence of silex. The nitrate dried, and redissolved in water, communicated to it but a very slight tinge. The silex, which had been separated, was perfectly white. The solution of nitrate of potash, tried in all ways with solutions of silver, mercury, and lead, even after having been considerably concentrated, did not exhibit the least sign of the presence of chrome. It appeared therefore, that the yellow colour of the solution was owing to a small quantity of platina taken up by the potash from the crucible in which the stone had been treated.

ilex.

No chrome.

Yellow colour from the platina of the crucible acted on by the potash.

Residuum.

The residuum, which had been separated from the green alkaline solution, was diluted in water, and supersaturated with muriatic acid, which dissolved it completely. This solution, which had a fine yellow colour, was evaporated to dryness, then diluted in acidulous water, and filtered. The silex, collected on a filter, was perfectly white, and was added to that before obtained.

Silex.

Muriatic solution.

The muriatic solution freed from silex was of a lemon colour. Being decomposed by ammonia in considerable excess, a very bulky brown precipitate was formed, which was collected on a filter. To the ammoniacal liquor oxalic acid was added, which produced a very copious precipitate of oxalate of lime. This was carefully filtered. Though the solution was colourless it was evaporated to dryness; and, after having been strongly heated, to volatilise part of the sal ammoniac, it was redissolved in water, and made to boil. On adding some potash a light black precipitate was obtained, which was carefully collected. This precipitate, while still wet, was dissolved in muriatic acid. The solution, which was yellow, was diluted with a pretty large quantity of water, and decomposed by saturated carbonate of potash, which occasioned some light flocks, that could not be collected without great trouble. The colour of these flocks was a greenish white; and they dissolved in ammonia,

Lime.

nia, which they tinged blue. This blue solution being evaporated, a little oxide was left, which could not be weighed; but it was treated with muriatic acid, and a slip of iron immersed in the solution, which did not become coated with copper. It is evident therefore, that this small quantity of matter was nickel. The liquor containing saturated carbonate of potash had retained a little manganese, but not the slightest trace of magnesia.

Nickel.

Manganese.

No magnesia.

I boiled the brown precipitate formed by ammonia in a solution of pure potash, which took up some alumine. This was separated, and treated with sulphuric acid. The alumine still retained a small quantity of silex and lime.

Alumine.

After having let the oxide of iron dry, I treated it with muriatic acid, and evaporated to dryness; when a small portion of silex again separated from it. All the silex was mixed together, and heated red hot. The muriatic solution of iron was decomposed by saturated carbonate of potash, filtered, and evaporated; when it still yielded a little oxide of manganese, without any magnesia being discoverable.

Silex.

More manganese, but no magnesia.

As I suspected, that the oxide of iron might still retain a little lime, or magnesia, I redissolved it in muriatic acid, and precipitated by oxalate of ammonia; but I obtained only a light yellow precipitate, which was oxalate of iron mixed with a little oxalate of lime. This I heated red hot, and then redissolved in muriatic acid. This solution I decomposed by ammonia, to get the oxide of iron; and precipitated the lime by oxalic acid. To precipitate all the iron from the solution to which oxalate of ammonia had been added, I employed the hidrosulphuret of ammonia, which formed a black precipitate. This was well washed, dried, calcined, then redissolved in muriatic acid, and precipitated afresh by ammonia. The solution decomposed by hidrosulphuret of ammonia contained no lime.

Iron and a little lime.

It follows from all the facts exhibited in this analysis, that the aerolite of Stannern contains silex, alumine, lime, iron, manganese, nickel, and sulphur; but I found neither magnesia nor chrome.

Component parts, according to Vauquelin.

The following are the proportions obtained from 100 parts.

Silex	50
Lime	12
Alumine	9
Oxide of iron	29
Oxide of manganese	1
Oxide of nickel, a slight trace, scarcely to be estimated at ·001	
Sulphur, an atom	
	101.

Muriatic acid found in it.

In another examination of this stone pretty evident traces of muriatic acid were found.

Differences in the two results.

These results differ a little from those obtained by Mr. Moser: 1st, in exhibiting no magnesia; 2dly, in containing nickel; 3dly, in a slight increase of weight, instead of loss; an increase necessarily consequent to the oxidation of the iron. This increase would have been still greater, if I could have calculated the quantity of sulphur disengaged by means of the hidrogen.

Differs from other aerolites,

This aerolite then is of a different species from those that have hitherto been analysed, since it contains neither magnesia, nor chrome, substances constantly found in the other aerolites; and in containing a considerable quantity of alumine, traces of which alone have been found in others.

Yet certainly one.

Yet this Moravian aerolite has all the external characters, that distinguish productions of this sort; and from the account given me it appears unquestionable, that it fell from the atmosphere.

Existence of alumine in meteoric stones doubted,

Note. It appears from a former paper in the Annales de Chimie, vol. LXIX, p. 280, that the Institute had entertained some doubts of the reality of the existence of alumine in meteoric stones, announced by Mr. Sage; see Journal, vol. XXIV, p. 190; because it had not occurred in the stones analysed by others. Mr. Vauquelin accordingly analysed a stone, that had fallen no long time before near Parma, but "could not discover in it at most above 0·0015 of alumine." He would willingly have analysed a specimen of the stone, in which Mr. Sage had found it; but this gentleman had none left, except the piece fashioned into a vase. Mr. Vauquelin however saw the products obtained by Mr. Sage, and was induced to think, that what he had considered as alum was a mixture of the sulphates of alumine, iron, and nickel. The subsequent analysis by Mr. Vauquelin however, given above, not only confirms the existence of alumine in these stones; but that, as Mr. Sage observed, it is in very different proportions; for the stone of Stannern contained more than either of the stones analysed by Mr. Sage. In the following extract of a letter from Mr. Klaproth we find farther confirmation of the fact. C.

but now confirmed.

VIII.

Extract of a Letter from Mr. KLAPROTH *to Mr.* GEHLEN*.

I HAVE just finished the analysis of a meteoric stone, that fell in the afternoon of the 13th of May, 1807, in the district of Juchnow, in the government of Smolenski, during a very heavy thunderstorm with a cloudy sky. It weighed 4 poods, or 120lbs. Berlin weight [124lbs. avoir.]. Like all meteoric stones it is covered with a slight grayish black crust. Interiorly it is of an earthy ashen gray. It is mixed with many little specks of pyrites, globules of iron, and many spots of brown oxide of iron. Its specific gravity is 3·7. It gave me as its component parts:

Analysis of another meteoric stone.

Metallic iron	17·60
Metallic nickel	0·40
Silex	38
Magnesia	14·25
Alumine	1
Lime	0·75
Oxide of iron	25
Loss, including a little sulphur, and a trace of manganese	3
	100.

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* Annales de Chimie, vol. LXX, p. 185.

It is somewhat remarkable, that I found alumine and lime in this meteoric stone, since no mention has been made of alumine in particular in the analysis of meteoric stones hitherto published. It is true Bartholdi, of Colmar, announced 0·17 of alumine in the stone of Ensisheim; but Vauquelin assures us, that he found this stone like all others.

That of Ensisheim contains alumine.

As I have a specimen of the stone of Ensisheim in my collection of meteorolites, I subjected a certain quantity to analysis, and found in it 1½ per cent of alumine.

Alumine in small quantity easily overlooked.

Terre verte.

It is very easy however, to overlook alumine, when it is contained in very small quantity in stones, as you justly observed on my analysis of the *terre verte* of Verona. Accordingly on repeating this analysis I found, that, by boiling the recently precipitated oxide of iron with a solution of caustic potash, I obtained 1·75 of alumine: this therefore is a principle to be added to those of the earth of Verona, and its weight must be deducted from that assigned to the oxide of iron.

Analyses seldom exact.

To speak generally I am far from pretending, that my analyses, even the most accurate, are perfectly just and exact; it is only by a careful repetition of them, that we can approach as near as possible to the truth.

Decomposition of the alkalis.

Tellurium will probably decompose water by attracting its hidrogen.

Among all the experiments of Ritter on Davy's decomposition of the alkalis I found the phenomena exhibited by tellurium most interesting. [See Journal, vol. XXIV, p. 318.] This property of tellurium leads us to expect, that we may accomplish the decomposition of water so that the hidrogen shall enter into combination, and the oxigen be separated in the form of gas. It is much to be wished, that this metal was more easy to be procured, that we might pursue these experiments; for at present we have no way of obtaining it but from the foliated ore of Nagyag.

Alkaline metals keep well under oil of turpentine, but convert it to a soap,

The products of soda and potash, which we obtained in the laboratory of the academy, in conjunction with Mr. Simon and Mr. Ermann, by employing the galvanic apparatus, have kept very well for three months under oil of turpentine, retaining their metallic lustre, particularly the large pieces. The oil of turpentine has acquired a transparent brown red colour,

lour, and a gelatinous consistence. It is a true Starkey's soap. When I poured fresh oil of turpentine on this soap to dilute it, and exposed it to the action of heat, there was a considerable extrication of gas, and the globules perceptibly diminished. This was owing to the hidrogen, which the soda had absorbed during the galvanic action, being separated again. When these small globules were taken out, and dried on paper, they changed their nature in a few seconds, and nothing was found but small heaps of soda.

IX.

On the Purification of Nickel by Means of Sulphuretted Hidrogen: by Mr. Robiquet, *Apothecary*[*].

HAVING treated a considerable quantity of speiss in the laboratory of Mr. Vauquelin, with a view to extract from it pure nickel, this process afforded me an opportunity of making some remarks, which I have thought necessary to make public, that they who engage in a similar pursuit may avoid the same errours. I shall give a brief account of the process I followed.

Attempt to obtain pure nickel from mispickel.

I took two pounds of speiss, which I treated directly with nitric acid diluted with two parts of water. I omitted the roasting commonly directed, from the danger of so large a quantity of arsenical vapour, and because the roasted ore is not acted upon so easily by acids. Thus the solution, which took place very quickly, was accompanied as usual with an evolution of nitrous gas; and the sulphur separated in proportion as it went on in a flocculent precipitate, which afterward united into a mass. The whole of the acid at 32° employed successively was 6lbs. 8oz.; and the residuum, which probably contained a pretty large quantity of arseniate of iron, weighed 7 oz. 6 drachms. I filtered the liquor, which I diluted with a large quantity of water in order

Treated with nitric acid, without previous roasting.

Arseniates of iron and cobalt.

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[*] Annales de Chimie, vol. LXIX, p. 285.

precipitated by potash.

order to dispose the arseniates of iron and cobalt to separate more fully; and as these two salts require more acid, to retain them in solution, than the arseniate of nickel, I accomplished their separation pretty easily, by adding gradually a solution of potash, but so as to leave an excess of acid in the liquor. The precipitate, that was formed, appeared to the eye to contain two substances: the lower stratum was composed of granular rosecoloured particles, which indicated arseniate of cobalt; the other, which was much less in quantity, consisted of finer particles, more flocculent, and of a dirty white. This I considered as arseniate of iron, but I found in it also some arseniate of copper. These two arseniates united and dried weighed ten ounces.

The solution treated with sulphuretted hidrogen.

The solution was treated with sulphuretted hidrogen, as directed by Mr. Proust, for the purpose of separating the copper, oxide of arsenic, and arsenic acid. In fact I obtained so copious a precipitate, that at the end of some days I was obliged to filter, because the tube, through which the gas was introduced, though of considerable diameter, was perpetually getting choked up. I continued to keep up the stream of sulphuretted hidrogen gas, as long as it occasioned any sediment in the solution.

Large quantity of precipitate.

This operation, which continued near a month, and consumed 6 lbs. 8 oz. of sulphuret of iron, yielded me on the whole 1 lb. 14 oz. of precipitate, which I separated at three different periods. The first was of a brown gray colour, and when dried was intermixed with spots of white; the second was more homogeneous, and deeper coloured; and the last was nearly black.

Treated with muriatic acid.

The quantity of these different sulphurets I had obtained appeared to me so exorbitant in respect to the speiss employed, that I was desirous of ascertaining their nature, in order to know, whether I had really separated nothing but copper and arsenic by this process. Accordingly I treated a portion of the first with muriatic acid. The solution was readily accomplished, and the sulphur separated. The filtered liquor was of a fine green, but precipitated white by prussiate of potash. Sulphuretted hidrogen did not render it turbid till after the lapse of a few moments. Having separated the arsenic by this means, I precipitated the whole

Precipitate treated with ammonia.

Residuum dissolved in muriatic acid.

whole of the solution with caustic potash. I then treated the precipitate, after it was well washed, with dilute ammonia, the proportion of which I increased, as long as the sapphire blue colour produced was increased in intensity. After macerating some time, I filtered. The undissolved portion had acquired a deep green colour, similar to the fine green of Scheele; and being treated with weak muriatic acid, it immediately became black, and dissolved, giving out oximuriatic acid gas, and imparting to the acid a fine pure rose colour. It was therefore oxide of cobalt at a maximum, which could not redissolve in the ammonia, as Mr. Thenard had observed. The ammoniacal solution, evaporated to dryness, let fall a light green powder, which comported itself as pure oxide of nickel with every reagent.

Precipitate at different periods differed only in proportions.

No copper.

What could occasion the precipitation of the nickel and cobalt by the sulphuretted hidrogen?

On the successive examination of the other two precipitates obtained by sulphuretted hidrogen, they were found to agree, except in the proportions, with the former. In the last the quantity of cobalt was scarcely perceptible. The second contained more than a tenth of arsenic in the state of an oxide not sulphuretted, which might be extracted by boiling water alone. The first contained much more nickel than the others. By the different processes which I employed to analyse the precipitates I constantly found sulphur, originating from the sulphuretted hidrogen, arsenic, nickel, cobalt, and not an atom of copper. This did not surprise me, because I knew, that some of these ores contain none: but what really puzzled me was, to account for the precipitation of the nickel and cobalt by the sulphuretted hidrogen. I thought at first this was owing to the process I had followed; but I was completely undeceived, after having analysed three specimens taken from different collections, and labelled "sulphuret of copper and arsenic obtained in the purification of nickel." These sulphurets were of the same nature as mine, only one of them contained in addition a little copper; yet they were produced by processes, in which alkali had not been employed for the separation of the arseniates of iron and cobalt as in mine. Obliged to look for some other source of the fact, I formed a series of conjectures, and made experiments accordingly,

cordingly, which it would be useless to enumerate, since they led to nothing interesting.

Not the addition of some foreign substance.

Experiment with a smaller quantity.

I could not suspect the existence of a substance, which, when united with nickel and cobalt, occasions their precipitation by sulphuretted hidrogen: for, when I redissolved these sulphurets in muriatic acid* mingled with a few drops of nitric acid, and I had separated from them nothing but sulphur, I could no longer precipitate them again by the same reagent. It must have arisen therefore from some circumstances peculiar to the operation itself: and in consequence, in order to examine it with more attention, I begun with a small quantity. The solution was accomplished in the same way by nitric acid diluted with twice its quantity of water by measure. The residual sulphur was separated by filtration, and I evaporated the solution gently to one fourth. I let it cool, to crystallize the oxide of arsenic, which was deposited in pretty large quantity. Having filtered the solution a second time, I evaporated it to a sirupy consistence: when in proportion as the excess of acid was carried off, the arseniate of iron was deposited, and formed a kind of gelatinous magma. Again I filtered and evaporated: and this I repeated to the fourth time, drawing off at each a little more acid.

Nitric solution evaporated and filtered repeatedly.

Different precipitates.

The first sediment obtained by evaporation was arseniate of iron in the form of a white powder: the second was a mixture of arseniate of cobalt and iron: the third appeared to be pure arseniate of cobalt. The last time I evaporated to dryness; and the portion that would not redissolve in water was arseniate of nickel.

Sulphuretted hidrogen added to the solutions.

At each filtration I took care to try the fresh solution with sulphuretted hidrogen, to endeavour to find the point at which the precipitation of the nickel took place; but I always obtained a precipitation of orpiment. The last alone yielded me a copious sediment of a blackish brown, which was found on examination to contain nickel, arsenic, and no copper. I then conceived, that the proportion of acid might have an influence in this precipitation. With this view I

Solution still acid.

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* I perceived, that, during this solution, the muriatic acid took up orpiment in a state of combination, which could be separated from it by the addition of water.

examined the state of my solution, and I saw, that it still reddened litmus paper perceptibly; and if I added a small portion of acid, the precipitate produced in it by sulphuretted hidrogen was no longer black, but of a fine yellow.

A predominance of acid necessary, to prevent the precipitation of the nickel.

Thus it appears to me demonstrated, that when the nitric acid is in too small proportion to exert a strong attraction for the oxide of nickel, it lets fall a certain quantity with the arsenic, till it becomes sufficiently predominant to counterbalance the combined action of the sulphur and arsenic on the nickel. Whence we perceive, that, to avoid this inconvenience, it is necessary to keep up a slight excess of acid in the liquor, and then we may operate with certainty.

X.

On the Experiments of Mr. CHENEVIX *and Mr.* DESCOTILS *on Platina*: *by Mr.* C. L. BERTHOLLET[*].

Mr. Chenevix combined mercury with platina.

MR. Chenevix published in the Philosophical Transactions a number of experiments on the combination of platina with mercury[†], from which it appeared, that this compound, in certain proportions, was capable of sustaining the strongest fire without the mercury being separated; that it was fusible; and that it bore a resemblance to palladium. But that celebrated chemist was desirous of his experiments being repeated and confirmed, particularly as Messrs Rose and Gehlen announced results contradictory to his. Accordingly I invited him to accompany Mr. Descotils to my laboratory at Arcueil.

Experiment pointed out by him.

Mr. Chenevix mentioned the following experiment, which he has described in the Annales de Chimie, vol. LXVI, p. 86, as best calculated to elucidate the subject.

Nitrate of mercury at a minimum added to muriate

"Let diluted nitric acid be boiled with a large quantity of metallic mercury, and the result will be a nitrate of mercury at a minimum of oxidizement. If this be poured into a mu-a muriatic solution of platina, a precipitate will be formed, composed of platina and mercury united with muriatic acid. Let this be washed, and reduced with a little borax in a crucible lined with charcoal, and a metallic button will be obtained. Dissolve this in nitro-muriatic acid, and it will be precipitated by green sulphate of iron."

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[*] Annales de Chimie, vol. LXVII, p. 86.

[†] See Journal, vol VII, p. 85, 176; and XI, p. 162 and 182.

of platina, and precipitate reduced with a little borax and charcoal.

These directions were followed, and a well melted button was obtained, the specific gravity of which was about 17.

Platina precipitated by green sulphate of iron.

Mr. Descotils having subjected to ebullition a mixture of green sulphate of iron and solution of platina, prepared so as to contain but little excess of acid, a copious precipitate fell down; so that this property belongs to platina itself.

Another difference remained; it was, that the platina precipitated with mercury possessed the property of being liquefiable by the action of fire, which indicated a considerable difference between the button obtained and platina that had not undergone the same preparation.

Platina fusible with the addition of borax.

In the interval between this meeting and the next, Mr. Descotils alone treated pure platina with borax in a crucible lined with lampblack, and obtained a button like that given by the mixed precipitate, and of similar specific gravity.

The button contained boracic acid.

By this experiment, which was repeated, it was proved, that platina is capable of entering into fusion by means of borax and charcoal. On dissolving part of the button, Mr. Descotils obtained from it boracic acid.

Platina combined with borax.

Platina therefore is capable of combining with the whole or part of the borax; and thus alloyed it has a perfectly metallic aspect, is hard and very brittle, and takes a crystalline form internally. Mr. Descotils had already observed similar phenomena with other metals.

Precipitate of platina and mercury not fusible without charcoal and borax.

The mixed precipitate urged with the most powerful forge fire, without the addition of charcoal and borax, did not enter into fusion; so that Mr. Chenevix acknowledged the property of liquefying was not owing to the mercury, with which the platina had been precipitated, but to the charcoal and borax; and he had himself remarked in his former paper, that platina might be fused by means of borax, in a crucible lined with charcoal no doubt, as will presently be seen.

There

Platina fused with charcoal alone.

There were two substances, the borax and charcoal, the action of which it was necessary to discriminate. In consequence Mr. Descotils treated pure platina with charcoal in a crucible, and observed, that the charcoal was sufficient, to produce its fusion.

Carburet of platina.

A button thus obtained at Arcueil had increased in weight a little more than 0·03, which were owing to the charcoal, that had combined with the metal. Its specific gravity was 18. This carburet was very hard, very brittle, shining, and lamellated internally.

Borax alone will not fuse platina.

Mr. Descotils observed, that borax without charcoal could not effect the liquefaction of platina; yet, by means of charcoal, borax, or more probably the boracic acid, enters into combination with platina. He will publish the experiments, to which these observations shall give rise, and which he intends to pursue.

XI.

On a singular Crystallization of the Diamond: by Mr. GUYTON-MORVEAU*.

Natural history of minerals a collection of facts.

THE natural history of minerals being only a collection of facts sufficiently established to be admitted without question, it is particularly important to confirm the reality of those that rarely meet our observation, in order to remove doubts, and cut short disputes, which tend but to retard the progress of science.

Crystallization of lapis lazuli.

Five years ago I published in the Annales de Chimie, a letter on the crystallization of lazulite, the lapis lazuli of the shops. The piece from which the accurate description was then made by Lermina, and which presents a perfect dodecaedron with rhombic faces, has been recognized by all who have seen it in my collection with the fine ultramarine, which Messrs. Desormes and Clement obtained from the mass in which this crystal was imbedded. Their opinion has been unanimously confirmed by comparing it with an ther fragment, in which likewise are found a number of grai of lazulite, enchased as it were in carbonate of lime, equal accompanied with sulphuret of iron, all of the same azu blue colour, and some of which pretty distinctly exhib angles and faces, notwithstanding their smallness.

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* Read to the Institute, March the 27th, 1809. Ann. de Chim. vol. LXX, p. 60.

Some have unreasonably doubted this.

Most of the mineralogists who have written since th publication have made no scruple to mention this regul figure under the article lazulite; though others have n even attempted to fix their opinion by an examination the crystal, a description of which I have published. The have concluded, that, being opake and mixed with pyrit and carbonate of lime, there was no proof of its bein really a lazulite; notwithstanding they agree with all othe mineralogists, that the fossile body which yields ultramarin is always opake, and commonly mixed with pyrites and carbonate of lime.

New crystal of the diamond.

Having had an opportunity of observing a crystalline forn of the diamond perhaps hitherto unique, I conceived th Class would receive some gratification from seeing it, an that this would be the most certain method of establishin its existence beyond all controversy.

Usual form of its crystals.

The primitive form of the diamond is known to be a regular octaedron. Most frequently it presents itself in spheroidal crystals or with curvilinear facets. It has been foun cubical, plano-convex, cylindroid: but it was not suspecte to be susceptible of that variety of form, which Romé d l'Isle termed *macle*, and Mr. Haüy has named *hemitrope* that is, where half of the crystal is turned back, so as t form reentering angles, as we see in some varieties of th ruby, feldspar, pyroxene, &c.

A macle or twin crystal.

Among the rough diamonds, which Mr. d'Arcet offere to sacrifice to the series of experiments undertaken b Messrs. Hachette, Clement, and myself, on the products their combustion, there was one, which we thought prope to set aside, as presenting the first example of a structur the prototype of which ought to be preserved.

This diamond is of the weight of 702 millig. [11 gr nearly]. Its specific gravity is 3·512.

The class will perceive at once, that it is formed of tw

demispheroid:

demispheroids, the deflected position of which, imperfectly terminated at one of the extremities, exhibits at the other the very decided reentering angles, that characterise the hemitrope.

XII.

Remarks on the Pechblende, an Ore of Uranium: by Mr. VAUQUELIN[*].

Mineral resembling gadolinite, or pechblende,

MR. de Lannoy, a dealer in minerals, and a skilful connoisseur in this branch of natural history, had in his collection several specimens of a black, shining, compact, heavy mineral, with a conchoidal fracture, the nature of which he did not know. Mr. Haüy, to whom he showed it, was in doubt between gadolinite and pechblende, which in fact it resembled more than any other substance.

proved to be the latter.

Having given me a piece of it to subject to chemical analysis, I soon found, that it was pechblende, or sulphuret of uranium. Though the experiments, by which I found what it was, have nothing new in themselves, I shall relate some of them, because they afforded me an opportanity of making a few remarks on the state in which uranium exists in this ore, and on the combinations it is capable of forming with oxigen.

Treated with muriatic acid.

The pechblende reduced to powder, and digested in muriatic acid moderately concentrated, dissolved without any perceptible effervescence, only a smell of sulphuretted hidrogen gas was emitted.

Solution filtered,

The solution had a very deep dirty green colour. Diluted with water and filtered, it left on the paper a small quantity of residuum; which, when washed and dried, exhibited all the properties of silex mixed with a little sulphur.

& evaporated.

During the evaporation, and more especially during the cooling, this concentrated solution deposited crystals of muriate of lead. These I separated by adding rectified alcohol, which dissolved the muriate of uranium without touching the muriate of lead.

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[*] Annales de Chimie, vol. LXVIII, p. 277.

The muriate of uranium dissolved in water and examined.

The muriate of uranium, after the alcohol was driven off by heat, was diluted with water, and subjected to the following experiments. 1. Pure and carbonated alkalis formed in this solution very deep bottle-green precipitates. These precipitates, particularly that from ammonia, turned black and shining when dried. 2. Prussiate of potash produced a deep chocolate brown precipitate. 3. Infusion of galls occasioned a greenish brown sediment, which in time changed of a yellowifh red in the upper part.

Precipitate by ammonia,

The precipitate formed by ammonia in the muriatic solution of uranium, though completely washed and dried in the open air, still retains much water and ammonia; for on

calcined,

being heated in a glass retort, it gave out a perceptible quantity of these two substances, and acquired a still deeper black colour.

and dissolved in nitric acid.

This matter, thus dried, dissolved readily in nitric acid diluted with water, even cold: but the solution, which had a green colour, immediately on being heated gave out a considerable quantity of nitrous vapours, and at the same time acquired an orange yellow colour.

The solution diluted,

The solution of this calcined oxide in nitric acid having been diluted with water, after some time it let fall a small quantity of oxide of iron. This ore of uranium therefore contains small quantities of lead, iron, sulphur, and silex. It appears to be the kind analysed by Mr. Klaproth under the name of pechblende of Joachimsthal.

& precipitated by alkalis,

This new solution of oxide of uranium in nitric acid was precipitated of an orange yellow by caustic alkalis, and of a pale yellow by alkaline carbonates, an excess of which redissolved the precipitate.

prussiate of potash, & galls.

Prussiate of potash and infusion of galls formed in this solution precipitates of a brown red much lighter than before.

Green oxide dissolved in muriatic acid,

The green oxide of uranium united with liquid oximuriatic acid soon destroyed its smell and colour; and the solution gradually assumed a yellow colour, like that effected by nitric acid.

and in other acids, crystallized.

The combinations of the green oxide of uranium with the sulphuric, nitric, muriatic, and acetic acids, evaporated to a proper degree, afford neutral salts, which crystallize each in its peculiar form, but all of a yellowish green colour.

Salts of the yellow neither neutral nor crystallizable.

The yellow oxide on the contrary combined with these acids never forms perfectly neutral salts, and does not crystallize in distinct and regular figures.

The yellow dissolves in alkalis, but the green does not.

The green oxide does not dissolve in alkaline carbonates: but the yellow oxide does in large quantity, as Mr. Klaproth first observed.

Two oxides of uranium therefore.

From these experiments it appears to me beyond a doubt, that uranium is susceptible of two degrees of oxidation, and this is principally the object I had in view to demonstrate here; one by which it forms a deep green oxide, the other by which it yields an orange yellow oxide. It exists in the first of these states in the pechblende; and it is in the second that it constitutes the yellow ore of uranium, such as that which Mr. Champeux discovered in the environs of Autun, in the department of the Upper Saone.

Oxide of the first degree in pechblende,

The pechblende of which I have given the analysis appears to me to contain uranium in the first degree of oxidation, for it dissolves in muriatic acid without any perceptible evolution of gas.

which is not a sulphuret.

I even doubt whether the sulphur contained in this ore be combined with the oxide of uranium, for the quantity is extremely small in proportion to that of this metal; and am more inclined to suppose, that it belongs to the lead, which also exists in it.

Klaproth of a similar opinion.

This reflection did not escape Mr. Klaproth, for he says in the second volume of his analyses: "I do not consider the black ore of uranium as a sulphuret, but as a metal combined with little oxigen. It is this nearly metallic state of the ore, that occasions the evolution of nitrous gas while it is dissolving in nitric acid." Mr. Klaproth however has not distinguished two species of the oxide.

The oxides analogous to those of iron

These two oxides comport themselves with the acids and alkalis nearly as the oxides of iron. Thus the green oxide of iron uniting with acids saturates them perfectly, and forms crystallizable salts; and does not dissolve in alkalis. The red oxide on the contrary, in its combination with acids, which it does not saturate, forms uncrystallizable salts; and it dissolves in concentrated subcarbonates.

From

bers. It is intended, that the Society shall be very select, consisting only of persons distinguished for their horticultural and botanical zeal. The Society purposes to publish Memoirs; and we doubt not that much useful information may thus be disseminated. The following gentlemen have been chosen office-bearers for 1810.

President. The Right Hon. the Earl of DALKEITH. Vice-Presidents. Sir JAMES HALL, Bart., M.P. Dr. RUTHERFORD, Prof. of Botany, Edin. Dr. COVENTRY, Prof. of Agriculture, Edin. ALEX. G. HUNTER, of Blackness. Mr. WALTER NICOL, and Mr. PATRICK NEILL, Secretaries. Mr. ANDREW DICKSON, Treasurer. Counsellors, Professional. Mr. THOMAS DICKSON, *Leith Walk.* Mr. JAMES MACDONALD, *Dalkeith.* Mr. EDWARD SANG, *Kirkaldy.* Mr. THOMAS SOMMERVILLE, *Botanic Garden.* Mr. JOHN FLETCHER, *Restalrig.* Mr. JOHN HAY, *Edinburgh.* Amateurs. Dr. DUNCAN, Sen. Dr. JAMES HOME. R. HODSHON CAY, Esq. GEORGE BRUCE, Esq. THOMAS HUTCHISON, Esq. JAMES SMITH, Esq.

Mathematical Repository.

Mr. T. Leybourn, of the Royal Military College, has just published the ninth number of his periodical work, entitled, The Mathematical Repository. It contains, beside various articles, Solutions to the Mathematical Questions proposed in the seventh number; and a series of New Questions, to which he solicits Answers from his correspondents, with a view to their being inserted in the eleventh number.—In publishing this work, the editor has in view, to promote the study of the various branches of the mathematics, by affording to the student an opportunity of cultivating his powers of invention in resolving problems which depend on its different theories: and also, to collect together and preserve the fruits of the studies of his ingenious correspondents, among whom he numbers some of the most skilful mathematicians in this country. The number here announced completes the second volume of the work. The many valuable articles in both these we would gladly enumerate, but that they are so numerous, they would occupy too much room.

Meteoric stone of Weston.

Prof. Woodhouse has analysed the meteoric stones, that fell at Weston, in Connecticut, on the 14th of October, 1807, 1807, and obtained from 100 parts, silex 50, iron 27, sulphur 7, magnesia 10, nickel 1, leaving a loss of 5. Some specimens of it, carried to France by col. Gibbs, were examined by Mr. Gillet-Laumont, who gives the following account of them.

"They contained rounded globules, ferruginous and brittle, of a blackish gray, and acquiring a dull metallic aspect on being rubbed with a smooth file. They were not very abundant, and appeared to be slightly attracted by the magnet.

Contained brittle iron,

"Small portions of malleable iron were diffused very plentifully through the stones. They were of irregular shapes, and very unequal in size; some black, but most of a shining silvery white; and easily cut with a steel instrument, like those contained in most aerolites. I separated a small, flat, triangular piece, about a quarter of an inch long, which I heated to different degrees, and afterward plunged into cold water, but could not make it harder.

malleable iron,

"On the face of one of the stones were some particles of mica; but as I could find none interiorly, I suppose they came from the ground on which it fell.

"Another of the specimens contained embedded in it a portion of a small body of the size of a pea, of a whitish gray colour, composed of smooth, shining lamellar facets, forming angles too small to be measured. It resembled a piece of broken feldspar. On endeavouring to detach a piece for the purpose of assaying it, the small mass immediately separated, leaving a cavity, which showed, that it was rounded before it was moulded in the stone. A particle of a very similar substance still exists in the stone; and there are some yellowish particles in the cavity from which this lamellar substance was taken.

and a lamellar substance.

"This substance scratched German sheet glass. It did not effervesce with nitric acid. Heated before the blowpipe it was immediately covered with a black enamel, which transuded in small globules, but the mass did not melt. I should have examined it farther, but I let it fall, and could not again find it.

Described.

"The aerolite of Weston therefore contained a substance, which was neither carbonate of lime nor feldspar, and I believe

Neither carbonate of lime nor feldspar.

lieve it is the first time that a lamellar substance, having the true elements of crystallization, has been mentioned as found in a stone fallen from the atmosphere."

Skeleton of the mammoth.

The skeleton of the mammoth found in the ice at the mouth of the Lena [See Journ. vol. XIX, p. 158], which has been for some time publicly exhibited at Moscow, is said to be intended for the museum of the Imperial Academy of Sciences at Petersburg. Prof. Tilesius has made forty drawings of the skeleton and its various parts, which he means to publish in folio, with observations. On some points he differs from Cuvier.

Cold of last winter.

The greatest cold of last winter observed at Moscow was in the night of the 11th of january. Mercury exposed to the open air in a cup by Dr. Rehmann was frozen so hard, that it could be cut with sheers, and even filed. Count Boutourline found the mercury in three thermometers withdrawn entirely into the ball, and frozen; but in another it was seen by himself, and four other persons from 6 o'Clock till half after at —35° R. [—46¾° F.]. Mr. Rogers, of Troitsk, is said to have seen it at —34° [—44½° F.] before it froze, and withdrew into the ball.

Conveyance of sound through solid bodies.

The aqueducts constructing at Paris have enabled Mr. Biot to make experiments on the propagation of sound through solid bodies on a larger scale, than had hitherto been done. The total length of the pipes was 951 met. [3118 feet]. A blow with a hammer at one extremity was heard at the other, producing two distinct sounds, the interval of which, measured in more than 200 trials, was 2·5″. The temperature was 11° [51·8 F.]. According to the experiments of the academy the time of the propagation of sound to this distance through the air should be 2·79″, at this temperature; from which if we deduct 2·5″, the interval observed, we have 0·29″ for the time the sound was in being propagated through the solid substance. This result was confirmed in another way. Two persons were stationed at the opposite extremities of the canal, each furnished with a half-second watch carefully compared, and each struck alternately with the hammer at intervals of 0, 15, 30, and 45 seconds. The time of the arrival of the two sounds was noted; and the sum of the numbers indicated by the watches gave double the time of the propagation by the solid substance, independent of the difference there might be between them. Thus the time of the transmission by the solid was found by repeated observations to be 0·26′, and of that by the air 2·76″. The first result differs from that given by the intervals of the sounds only 0·03″; and the second differs from the time deduced from the observations of the academy juſt as much: an agreement that appears to confirm the results.

Mr. Biot likewise observed, that at this distance the lowest voice could be heard perfectly from one end to the other, and with sufficient distinctness to keep up a conversation.

Native green oxide of chrome?

Mr. Leschevin, chief commissary of gunpowder and salt-petre, has sent from Dijon to the Council of Mines a collection of specimens of rocks, interesting on account of the parts coloured green that they contain. Several pieces of this stone, and a siliceous breccia, improperly called chalcedony of Creuzot, containing the same substance, had been found in abundance on the road; and Messrs. Guyton and le Lievre had ascertained, that the green colour was not owing to copper; but it was not known whence they came. After much search Mr. Leschevin discovered these green rocks in three contiguous mountains, and found that they were coloured by oxide of chrome, combined in greater or less quantity with silex, alumine, &c. On one of these mountains he met with the graphic granite, which several authors have mentioned as accompanying the emerald; and he intends to search for this stone also, which Mr. Vauquelin has discovered to be sometimes coloured with chrome. Since Mr. Drappier has shown, that chrome united with lead makes the most beautiful of all yellows, this may turn to considerable account.

Electric repulsion in the inverse ratio of the distance simply.

Mr. Simon, of Berlin, has been making some experiments on the true law of electric repulsion. Coulomb appears to have established this law by means of the torsion of wire in his electrical balance, that the electrical repulsion is in the inverse ratio of the square of the distance. To prove this law to his audience by a more simple and firm apparatus, Mr. Simon constructed a pair of scales, all the parts of which were made of glass, and coated with gum lac. Though inferior in sensibility to Coulomb's apparatus, it appears sufficiently sensible for experiments of this kind, since each degree of deviation of the tongue of the balance from 0 was equal to the weight of 0·04 of a grain. The result of Mr. Simon's experiments, the circumstances of which he varied in every possible way, was, that the electric repulsion was in the simple inverse ratio of the distance. In trials with the gold leaf electrometer this law was established with still more precision than in those which he made with the pith balls. It is to be observed, that Volta has always denied the truth of Coulomb's law, and asserted, that experiments with the electrophorus show the electric repulsion and attraction to be simply in the inverse ratio of the distance.

Greatest density of water.

Mr. Tralles has found by a very simple experiment, that the temperature at which water possesses the greatest density is at 39·83° F., or 4·35 of the centesimal thermometer; and he conceives this point, being more fixed than that of congelation, should be taken as the 0.

Standard of specific gravities.

Considering pure water at this density too as the proper unit for specific gravities, Mr. Trallas has given proper formulæ and a table for calculating specific gravities from this unit according to the different heights of the barometer and thermometer. Mr. Karsten has adopted these formulæ in calculating the specific gravities of minerals from his own experiments, with more accuracy than has usually been done, for the new edition of his Mineralogical Tables just published.

Answer to Dalton.

In another paper in Gilbert's *Annales der Physick*, entitled "Principles of Areometry, exhibited in the most general manner, and applied to the Vapour of Water, as a Critique on the Hypothesis of Dalton, and on some Calculations of the Density of the Vapour of Water," Mr Tralles demonstrates the insufficiency and erroneousness of Mr. Dalton's hypothesis of the mode in which elastic fluids mix with each other, and the constitution of the atmosphere. From the experiments he quotes the specific gravity of the vapour of water is to that of dry air as 1 to 1·45, in equal circumstances. From the results of the experiments of Biot and Arrago compared with those of Gilpin, he finds the density of dry air

Specific gravity of vapour.

dry air, and mercury.

air under a pressure of 0·76 m. [29·9 inches], and in the latitude of 45°, = 0·00129918 of the density of water at the temperature of melting ice, under equal circumstances, and = 0·0012770 of water at its greatest density. The specific gravity of mercury is 13·59925 at the former standard, and 13·59655 at the latter.

Plant that produces gum ammoniacum.

The plant that furnishes gum ammoniacum is not known, but it was supposed to be of the umbelliferous kind from the seeds frequently found mixed with it. Mr. Willdenouw however has been so fortunate as to get the seeds to germinate, and of the plant produced he has made a new genus, calling it *heracleum gummiferum*. As the root of this plant however contains no milky juice, he thinks we cannot yet consider it as certainly the plant that produces the ammoniacum.

Mode of preparing benzoic acid.

Mr. Suersen has given the following mode of preparing benzoic acid. Boil four ounces of benzoin in powder with three drachms of carbonate of soda in a sufficient quantity of water for an hour. Take out the benzoin, powder it afresh, and boil it again for half an hour in the same liquor. After several alternate boilings and powderings the soda will be entirely saturated with it, and five drachms of very pure benzoic acid may be precipitated from it by means of sulphuric acid; which is at the rate of 2½ ounces from a pound, or nearly one sixth.

Castor oil soluble in alcohol.

Mr. Rose had observed, that castor oil was completely soluble in alcohol. Mr. Bucholz has confirmed this, and says they unite in any proportion. Hence its sophistication with any fat oil may readily be detected; for these, though not completely insoluble in alcohol, do not mix with it except in very small quantities. Thus 60 drops of alcohol dissolve 2 drops of oil of almonds, 2 of poppy oil, 1 of rape oil, and 3 of old linseed oil. With the assistance of heat they would dissolve more.

Medical lectures.

Dr. Buxton's spring course of Lectures on the Theory and Practice of Medicine will be commenced about the middle of January, 1810, at the Medical Theatre, London Hospital.

METEOROLOGICAL JOURNAL,

For DECEMBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

NOV. Day of	THERMOMETER.				BAROMETER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.		Day.	Night.
27	42°	38°	43°5	32°	29·63	Fair	Rain
28	34	30	41	32	29·94	Cloudy	Cloudy
29	31	36	41	31	29·88	Ditto	Ditto
30	33	43	44·5	41	29·87	Fair	Fair
DEC.							
1	42	38	45	32	29·30	Ditto	Rain
2	37	37	43	33	29 69	Ditto	Fair
3	38	47*	47·5	43	29·92	Rain	Rain
4	44	41	47	37	29·50	Hail	Cloudy
5	38	43	44·5	42	29·88	Fair	Ditto
6	47	52	53	50	30·02	Cloudy	Ditto
7	†54·5	44	54·5	37	29·84	Rain	Fair
8	39	45	47	42	30·24	Fair	Cloudy
9	45	52	52	44·5	29·66	Cloudy	Rain
10	45	41	46·5	36	29·18	Fair	Ditto
11	38	40	45	37	29·37	Ditto	Fair
12	46	41	46·5	36	28·96	Rain	Cloudy
13	40	36·5	42·5	32	29·20	Ditto	Fair
14	34·5	36	42	36	29·40	Fair	Cloudy
15	40	36·5	43·5	33	28 76	Cloudy	Ditto
16	35·5	41·5	43	38	28·83	Fair	Rain
17	34	42	44	39	28·68	Rain	Cloudy
18	40	44*	44	37·5	28·32	Ditto	Ditto
19	41	42	43	37	29·28	Ditto	Fair
20	39	42·5	44	35	29·75	Fair	Rain
21	36·5	40	43·5	37	29·75	Ditto	Fair
22	39·5	41	44	37	29·95	Ditto	Ditto
23	38	39	41	32	29·90	Ditto	Ditto
24	34·5	35	38·5	31	29·83	Snow	Ditto
25	37	44*	44	36	30,00	Rain	Rain
26	39	41	42	36	29 76	Ditto	Ditto
27	40	41	43	35	29·80	Ditto	Cloudy

* Maximum of the Thermometer in the evening.

† Maximum of the Thermometer at 9 A. M.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1810.

ARTICLE I.

On Aerial Navigation. By Sir GEORGE CAYLEY, *Bart.*

HAVING, in my former communication*, described the general principle of support in aerial navigation, I shall proceed to show how this principle must be applied, so as to be steady and manageable.

Descent by a parachute.

Several persons have ventured to descend from balloons in what is termed a parachute, which exactly resembles a large umbrella, with a light car suspended by cords underneath it. Mr. Garnerin's descent in one of these machines will be in the recollection of many; and I make the remark for the purpose of alluding to the continued oscillation, or want of steadiness, which is said to have endangered that bold aeronaut. It is very remarkable, that the only machines of this sort, which have been constructed, are nearly of the worst possible form for producing a steady descent, the purpose for which they are intended. To render this

Of the worst possible form for a steady descent.

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* See Journal, vol. XXIV, p. 164.

Stability of a boat.

subject more familiar, let us recollect, that in a boat, swimming upon water, its stability or stiffness depends, in general terms, upon the *weight* and distance from the centre of the section elevated above the water, by any given heel of the boat, on one side; and on the *bulk*, and its distance from the centre, which is immersed below the water, on the other side; the combined endeavour of the one to fall, and of the other to swim, produces the desired effect in a well constructed boat. The centre of gravity of the boat being more or less below the centre of suspension is an additional cause of its stability.

Effect of a parachute, as commonly made,

Let us now examine the effect of a parachute represented by A B, Fig. 1, Pl. III. When it has heeled into the position *a b*, the side *a* is become perpendicular to the current, created by the descent, and therefore resists with its greatest power; whereas the side *b* is become more oblique, and of course its resistance is much diminished. In the instance here represented, the angle of the parachute itself is 144°, and it is supposed to heel 18°, the comparative resistance of the side *a* to the side *b*, will be as the square of the line *a*, as radius, to the square of the sine of the angle of *b* with the current; which, being 54 degrees, gives the resistances nearly in the ratio of 1 to 0·67; and this will be reduced to only 0·544, when estimated in a direction perpendicular to the horizon. Hence, so far as this form of the sail or plane is regarded, it operates directly in opposition to the principle of stability; for the side that is required to fall resists much more in its new position, and that which is required to rise resists much less; therefore complete inversion would be the consequence, if it were not for the weight being suspended so very much below the surface, which, counteracting this tendency, converts the effort into a violent oscillation.

and as it ought to be made.

On the contrary, let the surface be applied in the inverted position, as represented at C D, Fig. 2, and suppose it to be heeled to the same angle as before, represented by the dotted lines *c d*. Here the exact reverse of the former instance takes place; for that side, which is required to rise, has gained resistance by its new position, and that which is required to sink has lost it; so that as much power operates to restore the equilibrium in this case, as tended to destroy it in the other: the operation very much resembling what takes place in the common boat*.

Basis of stability in aerial navigation.

This angular form, with the apex downward, is the chief basis of stability in aerial navigation; but as the sheet which is to suspend the weight attached to it, in its horizontal path through the air, must present a slightly concave surface in a small angle with the current, this principle can only be used in the lateral extension of the sheet; and this most effectually prevents any rolling of the machine from side to side. Hence, the section of the inverted parachute, Fig. 2, may equally well represent the cross section of a sheet for aerial navigation.

Principle of stability in directing the path of the machine.

The principle of stability in the direction of the path of the machine, must be derived from a different source. Let A B, Fig. 3, be a longitudinal section of a sail, and let C be its centre of resistance, which experiment shows to be considerably more forward than the centre of the sail. Let C D be drawn perpendicular to A B, and let the centre of gravity of the machine be at any point in that line, as at D. Then, if it be projected in a horizontal path with velocity enough to support the weight, the machine will retain its relative position, like a bird in the act of skimming; for, drawing C E perpendicular to the horizon, and D E parallel to it, the line C E will, at some particular moment, represent the supporting power, and likewise its opponent the weight; and the line D E will represent the retarding power, and its equivalent, that portion of the projectile force expended in overcoming it: hence, these various powers being exactly balanced, there is no tendency in the machine but to proceed in its path, with its remaining portion of projectile force.

Remarkable

The stability in this position, arising from the centre of circumstance aiding this stability.

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* A very simple experiment will show the truth of this theory. Take a circular piece of writing paper, and folding up a small portion, in the line of two radii, it will be formed into an obtuse cone. Place a small weight in the apex, and letting it fall from any height, it will steadily preserve that position to the ground. Invert it, and, if the weight be fixed, like the life boat, it rights itself instantly.

gravity being below the point of suspension, is aided by a remarkable circumstance, that experiment alone could point out. In very acute angles with the current it appears, that the centre of resistance in the sail does not coincide with the centre of its surface, but is considerably in front of it. As the obliquity of the current decreases, these centres approach, and coincide when the current becomes perpendicular to the sail. Hence any heel of the machine backward or forward removes the centre of support behind or before the point of suspension; and operates to restore the original position, by a power, equal to the whole weight of the machine, acting upon a lever equal in length to the distance the centre has removed.

A rudder necessary similar to the tail of a bird.

To render the machine perfectly steady, and likewise to enable it to ascend and descend in its path, it becomes necessary to add a rudder in a similar position to the tail in birds. Let F G be the section of such a surface, parallel to the current; and let it be capable of moving up and down upon G, as a centre, and of being fixed in any position. The powers of the machine being previously balanced, if the least pressure be exerted by the current, either upon the upper or under surface of the rudder, according to the will of the aeronaut, it will cause the machine to rise or fall in its path, so long as the projectile or propelling force is continued with sufficient energy. From a variety of experiments upon this subject I find, that, when the machine is going forward with a superabundant velocity, or that which would induce it to rise in its path, a very steady horizontal course is effected by a considerable depression of the rudder, which has the advantage of making use of this portion of sail in aiding the support of the weight. When the velocity is becoming less, as in the act of alighting, then the rudder must gradually recede from this position, and even become elevated, for the purpose of preventing the machine from sinking too much in front, owing to the combined effect of the want of projectile force sufficient to sustain the centre of gravity in its usual position, and of the centre of support approaching the centre of the sail.

Farther use of the rudder.

The elevation and depression of the machine are not the only purposes, for which the rudder is desigaed. This appendage must be furnished with a vertical sail, and be capable of turning from side to side, in addition to its other movements, which effects the complete steerage of the vessel.

Experiments have been made on a large scale.

All these principles, upon which the support, steadiness, elevation, depression, and steerage, of vessels for aerial navigation, depend, have been abundantly verified by experiments both upon a small and a large scale. Last year I made a machine, having a surface of 300 square feet, which was accidentally broken before there was an opportunity of trying the effect of the propelling apparatus; but its steerage and steadiness were perfectly proved, and it would sail obliquely downward in any direction, according to the set of the rudder. Even in this state, when any person ran forward in it, with his full speed, taking advantage of a gentle breeze in front, it would bear upward so strongly as scarcely to allow him to touch the ground; and would frequently lift him up, and convey him several yards together.

The best mode of producing the propelling power only remains.

The best mode of producing the propelling power is the only thing, that remains yet untried toward the completion of the invention. I am preparing to resume my experiments upon this subject, and state the following observations, in the hope that others may be induced to give their attention towards expediting the attainment of this art.

Flight of birds.

The act of flying is continually exhibited to our view; and the principles upon which it is effected are the same as those before stated. If an attentive observer examines the waft of a wing, he will perceive, that about one third part, toward the extreme point, is turned obliquely backward; this being the only portion, that has velocity enough to overtake the current, passing so rapidly beneath it, when in this unfavourable position. Hence this is the only portion that gives any propelling force.

To make this more intelligible, let A B, Fig. 4, be a section of this part of the wing. Let C D represent the velocity of the bird's path, or the current, and E D that of the wing in its waft: then C E will represent the magnitude and direction of the compound or actual current striking the under surface of the wing. Suppose E F, perpendicular to A B, to represent the whole pressure; E G being parallel to the horizon, will represent the propelling force; and G F, perpendicular to it, the supporting power. A bird is supported as effectually during the return as during the beat of its wing; this is chiefly effected by receiving the resistance of the current under that portion of the wing next the body where its receding motion is so slow as to be of scarcely any effect. The extreme portion of the wing, owing to its velocity, receives a pressure downward and obliquely forward, which forms a part of the propelling force; and at the same time, by forcing the hinder part of the middle portion of the wing downward, so increases its angle with the current, as to enable it still to receive nearly its usual pressure from beneath.

Flight of the common rook.

As the common rook has its surface and weight in the ratio of a square foot to a pound, it may be considered as a standard for calculations of this sort; and I shall therefore state, from the average of many careful observations, the movements of that bird. Its velocity, represented by C D, Fig. 4, is 34·5 feet per second. It moves its wing up and down once in flying over a space of 12·9 feet. Hence, as the centre of resistance of the extreme portion of the wing moves over a space of 0·75 of a foot each beat or return, its velocity is about 4 feet per second, represented by the line E D. As the wing certainly overtakes the current, it must be inclined from it in an angle something less than 7° for at this angle it would scarcely be able to keep parallel with it. unless the waft downward were performed with more velocity than the return; which may be and probably is the case, though these movements appear to be of equal duration. The propelling power, represented by E G, under these circumstances, cannot be equal to an eighth part of the supporting power G F, exerted upon this portion of the wing; yet this, together with the aid from the return of the wing, has to overcome all the retarding power of the surface, and the direct resistance occasioned by the bulk of the body.

Very acute angles differ but little in their resistance.

It has been before suggested, and I believe upon good grounds, that very acute angles vary little in the degree of resistance they make under a similar velocity of current.

Hence it is probable, that this propelling part of the wing receives little more than its common proportion of resistance, during the waft downward. If it be taken at one third of the whole surface, and one eighth of this be allowed as the propelling power, it will only amount to one twenty-fourth of the weight of the bird; and even this is exerted only half the duration of the flight. The power gained in the return of the wing must be added, to render this statement correct, and it is difficult to estimate this; yet the following statement proves, that a greater degree of propelling force is obtained, upon the whole, than the foregoing observations will justify. Suppose the largest circle that can be described in the breast of a crow, to be 12 inches in area. Such a surface, moving at the velocity of 34·5 feet per second, would meet a resistance of 0·216 of a pound, which, reduced by the proportion of the resistance of a sphere to its great circle (given by Mr. Robins as 1 to 2·27) leaves a resistance of 0·095 of a pound, had the breast been hemispherical. It is probable however, that the curve made use of by Nature to avoid resistance, being so exquisitely adapted to its purpose, will reduce this quantity to one half less than the resistance of the sphere, which would ultimately leave 0·0475 of a pound as somewhat approaching the true resistance. Unless therefore the return of the wing gives a greater degree of propelling force than the beat, which is improbable, no such resistance of the body could be sustained. Hence, though the eye cannot perceive any distinction between the velocities of the beat and return of the wing, it probably exists, and experiment alone can determine the proper ratios between them.

The act of flying requires less exertion than commonly supposed.

From these observations we may, however, be justified in the remark—that the act of flying, when properly adjusted by the Supreme Author of every power, requires less exertions than, from the appearance, is supposed.

Brompton, Nov. 6th, 1809.

(*To be continued in our next.*)

H.

II.

Farther Remarks on Respiration, in Answer to J. F. By Mr. ACTON.

To Mr. NICHOLSON.

MY DEAR SIR,

Mr. Acton's motives for his experiments on germination and respiration

AS I am not conscious of having been influenced by any worse motive than the love of inquiry and truth in the prosecution of the experiments on germination and respiration, which you have done me the honour of inserting in your valuable Journal, I the more readily notice the ramarks of your correspondent J. F. in the last number*, being persuaded they were made from the purest intentions of doing justice to the talents and exertions of a very respectable individual, and correcting what may have appeared to him to have the semblance of misrepresentation.

in answer to Mr. Ellis.

I entreat it may be understood, that in speaking of Mr. Ellis's work nothing could have been farther from my thoughts, than any wish to lessen its merit, or detract from its value in the public estimation. Mr. Ellis thought proper to attempt the establishment of a new fundamental doctrine in chemical physiology; and for my own part being warmly attached to the more plain and simple manner of explaining the same phenomena, I presumed to lift up my feeble voice against the innovation, with what succeſs cannot at present perhaps be determined.

Points in question.

Opinion of Mr. Ellis.

Upon referring to Mr. Ellis's work and my papers, I believe it will be found, that the question at issue is precisely this. Do seeds, plants, and animals, during their respective functions of germination, vegetation, and respiration, absorb oxigen gas and emit carbonic acid gas? Or is solid carbon given out, and by uniting to the oxigen gas exteriorly to the seed, plant, or animal, does it form the carbonic acid gas produced? Mr. Ellis is of the latter opinion, and his work is written expressly to demonstrate it.

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* Vol. XXIV. p. 307.

He quotes a great number of experiments, and institutes a few of his own in support of it. Among those quoted is that of Bichat alluded to by J. F. Now the general tendency of Mr. Ellis's work, as far as it concerns respiration, is to prove the impossibility of oxigen gas being absorbed by the blood in the lungs during respiration. In my paper I observed, it was wonderful that Mr. Ellis should adduce the experiments of Bichat in support of this proposition, when Bichat stated it as affording a "proof of the passage of air into the blood through the lungs, in addition to that of healthy respiration." And I now add, is it not also *very unaccountable*, how Mr. Ellis should have overlooked those circumstances, which must destroy all analogy between the absorption of air in the lungs, and the injection of air into the vessels?

Bichat's experiment to prove the passage of air into the blood.

I very much hope neither your correspondent J. F., nor any of your readers, will suspect me of entertaining so preposterous a notion as that of the oxigen gas entering the blood vessels and continuing there in an aeriform state. On the contrary I have expressly declared, that death must be the consequence of such an event, no doubt by its mechanical action in impeding the circulation of the blood. I contend, the oxigen gas is absorbed chemically by the blood, which thus becomes changed from venous to arterial: the air consequently loses its elasticity and mechanical agency, as much as when it is combined with a metal or base to form an oxide or acid.

But the oxigen does not remain as a gas, it is chemically combined with the blood.

Mr. Ellis appears to have rested his principal argument against the absorption of oxigen gas by the blood upon what he conceives to be the impossibility of any chemical attraction taking place between them, on account of the interposed membrane, which separates the air in the lungs from the blood in the vessels.

Mr. Ellis supposes, the intervening membrane prevents this union.

To prove the contrary of this, suffer any small animal to die in oxigen gas, and immediately lay bare the thoracic viscera; observe them carefully for some time, and you will perceive a gradual change take place in the colour of the blood, in the lungs particularly. Compare this with another just opened, and the difference will be more obvious. Also examine the brain of an animal recently dead, the veins of the

The contrary proved

by the absorption of oxigen when a membrane intervenes.

the pia mater will appear quite purple; but after a little time they will begin to assume a degree of floridity, which can be accounted for in no other way than by the absorption of oxigen gas. Here then it seems the membranes are no positive impediment to its absorption. And if this so readily happen in dead animals, with how much more facility must it occur in those possessing vitality?

Death from injecting air into a vein no proof, that it cannot be absorbed by the blood in the lungs.

If an animal be killed by injecting air into the jugular or any other vein, it is not fair to infer the nonabsorption of oxigen gas by the blood through the lungs; in the former instance the point of contact is very trifling, and the absorption of course limited; neither is there any mode by which the carbonic acid gas can be emitted. The healthy functions of the vessels therefore must be destroyed, and the animal die. In the lungs it is very different. The human lungs expose an immense surface, not less than 21907 square inches, or ten times more than the whole body; and it is obvious, that the chemical action between the air and the blood must be considerably influenced by the surface of contact.

Difficulty to account for the formation of carbonic acid.

It is not so difficult to prove the absorption of oxigen gas by the blood, as to account satisfactorily for the formation of the carbonic acid gas emitted. That it ought to be considered excrementitious, and as the effect of the absorption of the oxigen gas, is my firm belief. Nature is so wise in her laws, that she will not permit continued repletion without some adequate evacuation. The oxigen gas no doubt acts as a nutrient and stimulant to the blood, the carbonic acid gas being the superfluous matter carried off. And surely it must be much more easily extricated in an aeriform than a solid state. My experiments on germination evinced with how much facility this gas is given out by seeds in a variety of instances, even when oxigen gas is not present; as also in spontaneous and putrid decomposition. Hence it appears, there need not be so much difficulty thrown in the way of rationally accounting for it, when produced by the healthy functions of plants and animals; for in the former case at any rate no reference can be made to the issuing of the solid carbon, and its uniting to the oxigen; nor can I see any sufficient reason, why the carbonic acid

Carbonic acid gas given out by seeds, when no oxigen gas is present.

may not be found interiorly in the animal or plant, as well in one case as in the other.

Mr. Ellis adduced two contradictory experiments.

I am certain it was not my wish to quote Mr. Ellis's work unfairly, and I do not conceive I have done so. However as your correspondent J. F. thinks I should have undertaken to show rather that Mr. Ellis contradicted himself, than that he perverted the experiments of Bichat, for his satisfaction I must inform him I could easily have done so, if it be admitted, that two experiments of an opposite nature, mentioned doubtless to prove the truth of the general proposition, can be considered a contradiction. In page 118 of his work is the following passage. "Nor when air "was forced down the windpipe of a dog in the experi"ments of Dr. Hales, was it able to pass into the pulmo"nary artery or veins;" and at page 128, "By forcing air "through the windpipe into the lungs with a syringe, and "confining it there, he (Mr. Bichat) has made it to enter "into the blood vessels."

Experiments of Messrs. Allen & Pepys

I cannot take leave of your correspondent without remarking upon the unfairness of his statement in his postscript. I appeal to yourself and readers if he have stated the question at issue with accuracy: and as to his leaning to the side of Mr. Ellis on account of the experiments of Messrs. Allen and Pepys and others, I can only say, the strong impression on my mind at the time I read them was directly the reverse. In the account of the experiments of Messrs. Allen and Pepys, at page 203 of the 22d vol. of your Journal, is the following passage. "In this recital of experiments, which "have occupied a considerable portion of time and atten"tion, we have endeavoured to give a plain statement of "facts, from which every one may draw conclusions for "himself." Clearly showing their own minds were by no means made up to decide upon the truth of any particular theory. And in the following page it runs thus: "When "respiration is attended with distressing circumstances, as "in the 14 and 15 experiments, *there is reason to conclude*, "*that a portion of oxigen is absorbed*; and in the last of "these experiments we may remark, that, as the oxigen "decreases in quantity, perception gradually ceases, and we "may which of course would be governed by light, heat, or moisture); that I shall not endeavour to refute it.

The contraction is owing to a stimulus.

We have now seen, that the contraction of the leaf of the dionæa muscipulâ takes place in consequence of the application of a stimulus; and that it does not seem probable, that this action can be explained on mechanical principles. What inference then must we draw? In my opinion we must again be forced to infer the same conclusion as we did with respect to the species of drosera, namely, that this plant is possessed of an inherent power, by which, when stimulated, it is enabled to contract; and if this inference be just, we would say, that the leaves possess irritability.

In considering the motions of this plant it must always be remembered, that it seems to move purely in consequence of the application of a stimulus.

Sensitive plant. Contraction of its leaves.

I shall next proceed to the mimosa pudica. It is well known, if we take a leaf of this plant similar to what is represented Pl. III, fig. 5, and then by means of a pair of scissars (completely dry), cut off half the pinnula A, this pinnula will contract at its joint either immediately, or in a few seconds; its neighbour, or opposite pinnula, B, closing at the same time, or soon after. The pinnulæ A and B having come into contact, there will be a pause or a short cessation of motion, but in the course of a few more seconds the next pair of pinnulæ, CC, will also shut up, and the same will happen with every pair of pinnulæ of that pinna successively; only with this difference, that the intervals between the shutting up of each pair of pinnulæ will be shorter, the farther they are from the pinnula that was cut. After the whole of the pinnulæ of this pinna have completely closed, and a little interval, then the joint D will become affected i. e. it will bend so as to allow the pinna to drop considerably. Nevertheless, the motion is often not so obvious in this joint as in that to be mentioned. A longer pause will now intervene, in some cases so long as to make us suppose that all motion is at an end; but at length, the joint E suddenly bends, and astonishes the beholder. The petiole F now instead of forming an acute angle with the stem above the joint, forms a very obtuse angle with it. We shall now have another cessation of motion, and then the joint H will slightly bend; then another pause; then a shutting up of the pair of pinnulæ, H, and so on with the other pinnulæ, till the whole pinna is closed. The motions however will not be so regular in this pinna, as they were in the other, for as the pinnulæ H approach they press forwards the next pair, and so on with all the rest. If we take another leaf similar to that of which I have given a sketch, and cut the pinnula of the pinna A, B, C, or D, fig. 6, in the same manner, a variety of beautiful experiments may be made; which will strike the experimenter with amazement, and excite an anxiety in his mind to ascertain the cause of this motion.

The motion not to be explained on mechanical principles.

The above related experiment I think sufficient for my present purpose; which is to endeavour to show, that these motions cannot be explained on any mechanical principles, and that they very probably depend on the irritability of the plant.

It does not appear, that the motion is occasioned by impulse: for a bit of the pinnula may be cut off almost without producing any motion. But allowing that a little motion were produced in the injured pinnula, it comes naturally, as a question why does the motion become so extensive? how is the impulse communicated to the origin of the petiole? These questions I believe will never be satisfactorily answered upon the principle mentioned.

Examination of Mrs Ibbetson's theory.

Camparetti's theory will be liable to nearly the same objections as that of Mrs. Ibbetson noticed below. I do not here intend to deny, that a structure similar to what Mrs. Ibbetson describes is to be found in the mimosa sensitiva; although I confess my doubts have increased of late. I am willing to allow, that all is accurate, and shall proceed now to inquire, whether by such a structure, acted upon by heat, light, or moisture, we could possibly explain the motions of the mimosa pudica. In the experiments related above, I presume no one would say, that moisture was the cause of the motion; as the scissars were quite dry. It is to be remembered also, that this plant will perform its motions under water. As there was no change of light, consequently this had no share in the effect. Besides, when moisture is produced in consequence of the abstraction of light, all the pinnulæ shut up at the same time: not in the regular

The motion not produced by moisture,

light,

darkness,

or change of temperature.

regular order mentioned in the experiment. Neither did the motion take place from change of temperature, for the temperature was not altered.

How then?

A great many questions will here suggest themselves, as, How does it happen, that the motion is produced? How does it become so extensive? How comes it that there are such regular motions and pauses? &c. I expect at some future period to be able to throw some light on this difficult subject, but in the mean time must beg leave again to quote Dr. Smith's words, and say, that " it is vain to attempt any mechanical solution of the phenomenon" above related, which would seem to depend on an " exquisite irritability" in the plant itself.

Sundew not affected by light, heat, moisture, or their opposites.

P. S. I might have added at the end of my last, that the leaves of the drosera rotundifolia and longifolia remain completely expanded during the hottest sunshine and driest weather; during the coldest and wettest weather*; during the greatest darkness, and finally during the brightest light of day. Here then, neither heat, cold, dryness, dampness, darkness, nor light in general at all effect the leaves; but if a foreign body is applied to the leaf so as to stimulate, then it will shut up in the manner related in my last. How will this agree with Mrs. Ibbetson's ideas?

IV.

Questions on the Study of the Mosses. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

How should the mosses be studied?

I Should consider myself obliged, if you should think the following questions of sufficient import to merit a place in your Journal. In commencing the study of the musci, should the generic divisions pointed out by Hedwig, and by Dr. Smith be followed? Or are these divisions too difficult for a beginner? Ought not distinctions similar to those of Hithering to be adopted, which are more obvious to a beginner? Is not a good introduction to the cryptogania class still wanted? How would such an introduction be received? and How should it be conducted?

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* This is to be taken in a limited sense, i. e. only during the expansion of the leaves, not during the cold of winter.

T. L. N.

V.

Analysis of the compact red Iron Ore in cubic Crystals from Toeschnitz in Thuringia. By Mr. Bucholz[*].

BEING at Ilmenau in june 1786, I had the pleasure of seeing my old friend, mine-counsellor Voigt, who showed me a mineralogical rarity lately discovered, a compact red ore of iron crystallized in cubes. He presented me with a specimen in which the crystals were still on their gangue, in order that I might analyse them: but on my remarking, that the crystals when separated would be in too small quantity, and might retain a little of the gangue, he gave me some perfectly pure separate crystals, and communicated to me the particulars of the situation where they were found.

Red iron ore in cubic crystals lately discovered

This species of minerals is found at Toeschnitz in Thuringia. The gangue of the crystals is compact red iron ore passing into a primitive argillaceous schist. The cut in which they were found has been given up, because the iron ore was too poor; and it is to be apprehended, that, when this is once filled, mineralogifts will no longer be able to procure it.

in Thuringia.

Its gangue.

It would be superfluous here to give a description of the compact red iron ore, which is to be found in all treatises on mineralogy, and agrees with this mineral; except that it is in perfectly regular cubes, differing in size from that of a lentil

Appearance.

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[*] Journal des Mines, vol. XXII, p. 435.

lentil to that of a pea. The smallest crystals are frequently fixed on the largest.

Lost nothing by heat.

a. A hundred grains of this substance, exposed for half an hour to a very brisk red heat, suffered no loss.

Treated with muriatic acid.

b. A hundred grains were powdered in an agate mortar. The powder had the same brown red colour of oxide of iron at a maximum heated redhot in the fire. It was boiled for a moment with concentrated muriatic acid of the spec. grav. of 1·165. No oximuriatic acid was formed during the solution. The whole was dissolved except a few small grains of quartz, which certainly were no constituent principles of the mineral, since they were not coloured by iron.

Few grains of quartz.

c. The solution *b*, which had the same colour as muriatic solutions of brown red oxide of iron, was divided into two equal portions, and one of these was subjected to the following trials.

No alumine,

α. Part of it was decomposed by caustic potash in excess, and the solution was boiled on the precipitate. After filtering and neutralizing with muriatic acid, caustic ammonia was added, which did not form the slightest cloud in the solution; but it would have become turbid, if it had contained any alumine or other substance dissolved by the caustic potash.

barytes, strontian,

β. Diluted sulphuric acid occasioned no precipitate in the first solution, whence it follows, that the precipitate contained neither barytes nor strontian.

lime,

γ. Another portion was decomposed by an excess of ammonia, and the precipitate separated by filtration. Oxalate of ammonia added to the colourless liquid did not render it in the least turbid. It contained no lime therefore.

magnesia,

δ. Another part of the first solution having been decomposed by ammonia, and the precipitate separated, the supernatant fluid was boiled with carbonate of soda; and as it underwent no change, it certainly contained no magnesia.

manganese or other substance.

ε. Muriate of barytes did not render the solution turbid.

d. The other half of the solution *b* was neutralized by ammonia, and the oxide of iron precipitated by succinate of ammonia: after which I sought in vain to detect manganese, lime, or any other substance, in the filtered liquor.

This mineral is consequently a pure oxide of iron crystallized in cubes; and according to my last researches it consists of 70·5 iron with 29·5 oxigen.

A pure oxide of iron.

VI.

Extract of a Letter from Mr. COUTELLE, *Subinspector at Reviews, to Mr.* GUYTON-MORVEAU, *on the Parabolic Lens of Rospini, purchased at Vienna for the French Government*[*].

THIS lens is about a metre [3 feet 3 in.] in diameter, and 2½ met. [8 f. 4 in.] focus. It is composed of two pieces of glass, united together by a hoop of iron, so as to form a hollow vessel capable of holding 80 or 90 quarts of spirit of wine.

Large burning lens,

It was made at Gratz, in Styria, by Rospini, a celebrated mechanist, for some alchemists. It was not cast, but softened by heat, and bent over a parabolic mould. Several pieces were broken before he succeeded, so that it cost originally 20 or 30 thousand francs [from 800 to 1200 guineas]. Mr. Coutelle paid for it 2900 florins [£338] in paper money.

made for some alchemists.

Mr. Jacquin of Vienna, and several men of science, who were witnesses to the experiments, say, that it burned a diamond in a few seconds, and fused platina in a few minutes. A button of platina weighing 29 grains was melted by it, and made in part to boil. The diameter of the focus does not appear to exceed four lines.

Its effects.

The lens, with the apparatus for placing the object of the experiment, fixed on a platform, and placed on an inclined plane mounted on a strong frame, is made to follow the course of the sun by means of machinery regulated by a pendulum beating seconds.

Made to move with the sun.

When made to incline toward the east or west it requires a counterpose, otherwise it would be liable to fall, being of the weight of 250 kilogr. [550 lbs avoird.].

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[*] Abridged from the Annales de Chimie, vol. LXIX, p. 92.

VII.

VII.

An Account of a Method of dividing Astronomical and other Instruments, &c. By Mr. EDWARD TROUGHTON.

(Concluded from page 18.)

True divisions to be made from the erroneous dots.

HAVING now completed the two first sections of my method of dividing; namely, the first, which consists of making 256 small round dots; and the second, in finding the errours of these dots, and forming them into a table; I come now to the third and last part, which consists in using the erroneous dots in comparison with the tabulated errours, so as ultimately to make from them the true divisions.

Subdividing sector described.

It will here be necessary to complete the description of the remaining part of the apparatus. And first, a little instrument which I denominate a subdividing sector presents itself to notice. From all that has hitherto been said, it must have been supposed, that the roller itself will point out, upon the limb of the instrument to be divided, spaces corresponding to others previously divided upon itself, as was done in setting off the 256 points: but, to obviate the difficulty of dividing the roller with sufficient exactness, recourse was had to this sector; which also serves the equally important purpose of reducing the bisectional points to the usual division of the circle. This sector is represented of half its dimensions by Fig. 5, Pl. I. It is formed of thin brass, and centered upon the axis at A, in contact with the upper surface of the roller: it is capable of being moved round by hand; but, by its friction upon the axis and its pressure upon the roller, it is sufficiently prevented from being disturbed by accident. An internal frame B B, to which the arc C C is attached, moves freely in the outer one, and by a spring D is pushed outwards, while the screw E, the point of which touches the frame B, confines the arc to its proper radius. The arc of this sector is of about four times greater radius than the roller, and upon it are divided the spaces which must be transferred to the instrument, as represented on a magnified scale by Fig. 4. Now, the angle of one of the spaces of the circle will be measured by sixteen times its angular value upon the sectorial arc, or 22° 30′; but this does not represent any number of equal parts upon the instrument, the subdivisions of which are to be 5′ each; for $\frac{1^\circ\,24'\,22''5}{5}$ is exactly 16⅞, therefore so many divisions are exactly equal to a mean space between the dots, the errours of which have been tabulated. Let, therefore, the arc of the sector be divided into 16 spaces of 1° 20′ each, and let a similar space at each end be subdivided into eight parts of 10′ each, as in Fig. 4; we shall then have a scale which furnishes the means for making the true divisions, and an immediate examination at every bisectional point.

Dividing by the engine.

I have always divided the sector from the engine, because that is the readiest method, and inferior to none in point of accuracy, where the radius is very short; but, as it is more liable than any other to centrical errour, the adjustment of the arc by the screw E becomes necessary: by that adjustment, also, any undue run in the action of the roller may be reduced to an insensible quantity*.

Division by lines preferable to dots.

When the utmost degree of accuracy is required, I give the preference to dividing by lines, because they are made with a less forcible effort than dots are; and also because, if any small defect in the contexture of the metal causes the cutter to deviate, it will, after passing the defective part, proceed again in its proper course, and a partial crookedness in the line will be the only consequence; whereas a dot, under similar circumstances, would be altogether displaced. But, on the other hand, where accuracy has been out of the question, and only neatness required, I have used dots; and I have done so, because I know that when a dot and the wire which is to bisect it are in due proportion to each other, (the wire covering about two thirds of the dot) the nicest comparison possible may be obtained. It may be farther observed, that division by lines is complete in itself; whereas that by dots requires lines to distinguish their value.

Apparatus for cutting the divisions.

On the upper side of Fig. 1 is represented the apparatus for cutting the divisions. It consists of three pieces J K L, jointed together so as to give to the cutter an easy motion for drawing lines directly radiating from the centre, but inflexible with respect to lateral pressure; *d d* are its handles. The cutting point is hidden below the microscope H; it is of a conical form, and were it used as a dotting point, it would make a puncture of an elliptical shape, the longer diameter of which would point towards the centre. This beautiful contrivance, now well known, we owe to the ingenuity of the late Mr. Hindley of York; it was borrowed by Mr. Ramsden*, and applied with the best effect to his dividing engine.

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* See note page 130.

Invented by Mr. Hindley.

It might have been mentioned sooner, that in the instance which I have selected as an example of my dividing, the operation took place when the season of the year, and the smoke of London, had reduced the day to scarcely six hours of effective light; and rather than confine my labours within such narrow limits, I determined to shut out the day-light altogether. Fig. 7 shows the construction of the lanterns which I used. A very small wick gave sufficient light, when kept from diverging by a convex lens; while the inclining nossel was directed down exactly upon the part looked at, and the light, having also passed through a thin slice of ivory, was divested of all glare. I enter into this description, because, I think, I never saw my work better, nor entirely to so much advantage as in this instance; owing, perhaps, to the surrounding darkness allowing the pupil of the eye to keep itself more expanded, than when indirect rays are suffered to enter it. The heat from a pair of these lanterns was very inconsiderable, and chiefly conducted along with the smoke up the reclining chimney.

Lanterns employed.

Preparation for cutting the divisions.

Previous to cutting the divisions, the parts now described must be adjusted. The cutting apparatus must be placed with the dividing point exactly at the place where the first line is intended to be drawn, and clamped, so that the adjusting screw may be able to run it through a whole interval. The microscope H must be firmly fixed by its two pillars *b b* to the main frame, with its micrometer head at *zero;* and with its only wire in the line of the radius, bisecting the first of the 256 dots. And it should be observed, that the cutting frame and this must not vary respecting each other, during the time that the divisions are cut; for any motion that took place in either would go undiminished to the account of errour. The microscope I is also fastened to the main frame; but it is only required to keep its position unvaried, while the divisions of the sector pass once under its notice; for it must have its wires adjusted afresh to these divisions at every distinct course. The microscope I has two wires, crossing each other at an angle of about 40°; and these are to be placed so as to make equal angles with the divisions of the sector, which are not dots, but lines. The sectorial arc must also be adjusted to its proper radius by the screw E, Fig. 5; *i. e.* while the main frame has been carried along the circle through a mean interval ſhown by H, the sector must have moved through exactly $16\frac{7}{8}$ of its divisions, as indicated by I *.

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* This I learned from that most accurate artist Mr. John Stancliffe, who was himself apprentice to Hindley.

Cutting the divisions.

Things being in this position; after having given the parts time to settle, and having also sufficiently proved the permanence of the micrometer H and the cutting frame with respect to each other, the first division may be made; then, by means of the screw for slow motion, carry the apparatus forward, until the next line upon the sector comes to the cross wires of I; you then cut another division, and thus proceed until the 16th division is cut, = 1° 20′. Now the apparatus wants to be carried farther, to the amount of $\frac{7}{8}$ of a division, before an interval is complete; but at this last point no division is to be made; we are here only to compare the division on the sector with the corresponding dot upon the instrument. This interval, however, upon the circle will not be exactly measured by the corresponding line of the sector, which has been adjusted to the mean interval, for the situation of the dot 1°·4 is too far back, as appears by the table of real errours, by — 4·8 divisions of the micrometer head. The range of the screw for slow motion must now be restored, the cross wires of H set back to — 4·8 divisions, and the sector moved back by hand, but not to the division 0, where it began before; for, as it left off in the first interval at $\frac{7}{8}$ of a division, it has to go forwards $\frac{1}{8}$ more before it will arrive at the spot where the 17th division of the instrument 1° 25′ is to be made, so that in this second course it must begin at $\frac{1}{8}$ short of 0. Go through this interval as before, making a division upon the circle at every one of the 16 great divisions of the sector; and H should now reach the third dot, allowing for a tabular errour of — 10·2 when the division $\frac{6}{8}$ths of the sector reaches the cross wires of I. It would be tedious to lead the reader through all the variety of the sector, which consists of eight courses; and it may be sufficient to observe, that at the commencement of every course, it must be put back to the same fraction of a division which terminated its former one; and that the wire of the micrometer H must always be set to the tabular errour belonging to every dot, when we end one interval and begin another. The eight courses of the sector will have carried us through $\frac{1}{32}$ part of the circle, 11° 15′, and during this time, the roller will have proceeded through half a revolution; for its close contact with the limb of the circle does not allow it to return with the sector, when the latter is set back at every course. Having in this manner proceeded, from one interval to another, through the whole circle, the micrometer at last will be found with its wire, at *zero*, on the dot from which it set out; and the sector, with its 16th division, coinciding with the wires of its microscope.

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* For the sake of simplicity, the account of the process is carried on as if the roller measured the mean interval without errour: But it was said (Page 3) that the roller, in a continued motion quite round the circle, would in some part of its course err by 30″ or more; therefore, when this is the case, an extreme run of the roller cannot agree with a mean interval of the circle nearer than $\frac{30''}{128} = 0{\cdot}23''$; and most probably this kind of errour will on some intervals amount to double that quantity. It therefore becomes matter of prudent precaution, to examine every interval previous to making the divisions; and where necessary, to adjust the sector, so that its arc may exactly measure the corresponding interval as corrected by the tabulated errours.

Advantages of this method.

Having now given a faithful detail of every part of the process of dividing this circle, I wish to remind the reader that, by verification and correction at every interval, any erroneous action of the roller is prevented from extending its influence to any distant interval. It will be farther observed, that the subdividing sector magnifies the work;

that by means of its adjustable arc, it makes the run of the roller measure its corresponding intervals upon the circle; and, without foreign aid, furnishes the means of reducing the bisectional intervals to the usual division of the circle. Farthermore, the motion of the wire of the micrometer H, according to the division of its head and corresponding table of errours, furnishes thé means of prosecuting the work with nearly the same certainty of success, as could have happened, had the 256 points been (which in practice is quite impossible) in their true places.

Why termed dividing by the eye.

Now the whole of my method of dividing being performed by taking short measures with instruments which cannot themselves err in any sensible degree, and, inasmuch as those measures are taken, not by the hand, but by vision, and the whole performed by only looking at the work, the eye must be charged with all the errours that are committed, until we come to cut the divisions; and, as in this last operation the hand has no more to do than to guide an apparatus so perfect in itself, that it cannot be easily made to deviate from its proper course, I would wish to distinguish it from the other methods by denominating it, DIVIDING BY THE EYE*.

The

Degree of accuracy of the sight & touch.

* I must here remark, that Smeaton has represented the greatest degree of accuracy that can be derived from vision, in judging of the coincidence of two lines, at $\frac{1}{4000}$ part of an inch. From this it may fairly be inferred, that he had not cultivated the power of the sight, as he had done that of the touch; the latter of which, with that ability which appeared in all his works, he rendered sensible to the $\frac{1}{60000}$ part of an inch. Were materials infinitely hard, no bounds could be set to the precision of contact; but taking things as they are, the different degrees of hardness in matter, may be considered as a kind of magnifying power to the touch, which may not unaptly be compared with the assistance which the eye receives from glasses. It is now quite common to divide the seaman's sextant to 10″, and a good eye will estimate the half of it; which, on an eight inch radius is scarcely $\frac{1}{10000}$ of an inch This quantity, small as it is, is rendered visible by a glass of one inch focal length; and such is the certainty with which these quantities are seen, that a seaman will sometimes complain that two pair of these lines will coincide at the same time; and this may happen, and yet no division of his instrument err by more than $\frac{1}{10000}$ part of an inch. All this is applicable to judging of the coincidence of *lines* with each other, and furnishes not the most favourable

Few persons capable of dividing originally.

The number of persons at all capable of dividing originally have hitherto been very few; the practice of it being so limited, that, in less than twice seven years, a man could hardly hope to become a workman in this most difficult art. How far I shall be considered as having surmounted these difficulties, I know not; but if, by the method here revealed, I have not rendered original dividing almost equally easy with what copying was before, I have spent much labour, time, and thought, in vain. I have no doubt indeed, that any careful workman who can divide in common, and has the ability to construct an astronomical instrument, will, by following the steps here marked out, be able to divide it, the first time he tries, better than the most experienced workman, by any former method.

Subdivision with the screw instead of the roller.

If, instead of subdividing with the roller, the same thing be performed with the screw, it will not give to dividing by the eye any very distinctive character. I have practised this on arcs of circles with success, the edge being slightly racked, the screw carrying forward an index with the requisite apparatus, and having a divided micrometer head; the latter answers to the subdividing sector, and, being used with a corresponding table of errours, forms the means of correcting the primitive points; but the roller furnishes a more delicate action, and is by far more satisfactory and expeditious.

Six feet circle for the Royal Observatory, dividing on its edge.

It is known to many, that the six feet circle, which I am now at work upon for our Royal Observatory, is to be divided upon a broad edge, or upon a surface at right angles to the usual plane of division: The only alterations, which will on this account be required, are, that the roller must act upon that plane which is usually divided upon; which

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vourable display of the accuracy of vision. But with the microscopes here described, where the wire bisects the image of a dot, or a cross wire is made to intersect the image of a line, by an eye practised in such matters, a coincidence may undoubtedly be ascertained to $\frac{1}{50000}$ part of an inch. I am of opinion, that as small a quantity may be rendered visible to the eye, as can by contact be made sensible to the touch; but whether Mr. Smeaton's $\frac{1}{60000}$ and my $\frac{1}{50000}$ be not the same thing, I will not determine; the difference between them, however, is what he would no more have pretended to feel, than I dare pretend to see.

roller, being elevated or depressed, may be adjusted to the commensurate radius without being made conical, as was necessary in the other case. The apparatus, similar to the other, must here be fixed immovably to the frame which supports the circle; its position must be at the vertex, where also I must have my station; and the instrument itself must be turned around its axis, in its proper vertical position, as the work proceeds. The above may suffice, for the present, to gratify those who feel themselves interested upon a subject, which will be better understood, if I should hereafter have the honour of laying before the Royal Society a particular description of the instrument here alluded to; a task which I mean to undertake, when after being fixed in the place designed for it, which I hope will be effected at no very distant period, it shall be found completely to answer the purposes intended.

A circle may readily be divided centesimally,

Should it be required to divide a circle according to the centesimal division of the quadrent, as now recommended and used in France, we shall have no difficulty. The 100° of the quadrent may be conveniently subdivided into 10 each, making 4000 divisions in the whole round. The 256 bisectional intervals, the two tables of errours, and the manner of proceeding and acting upon them will be exactly the same as before, until we come to cut the divisions; and for this purpose we must have another line divided upon the sector. For $\frac{1}{4000}$ part of the circle being equal to $5'{\cdot}4$ of the usual angular measure $\frac{1^\circ\,24'\,22''{\cdot}5}{5{\cdot}4} = 15\frac{5}{8}$ divisions; and just so many will be equivalent to one of the intervals of the circle. The value of one of the great divisions of the sector will be 1° 26′ 24″, and that of the $\frac{1}{8}$ parts, which are to be annexed to the right and left as before, will be 10′ 48″, therefore divisible by the engine. Should any astronomer choose to have both graduations upon his instrument, the additional cost would be a mere trifle, provided both were done at the same time.

and in the common way at the same time.

Straight lines may be divided on the same principle.

It must already have been anticipated, that dividing by the eye is equally applicable to strait lines as it is to circles. An apparatus for this purpose should consist of a bar of brass, Aparatus for the purpose.

brass, three quarters of an inch thick, and not less than three inches broad; six feet may do very well for the length; it may be laid upon a deal plank strengthed by another plank screwed edgewise on its lower surface. The bar should be planed, on both its edges and on its surface, with the greatest exactness; and it will be better, if it has a narrow slip of silver, inlaid through its whole length, for receiving the dots. An apparatus nearly similar to the other should slide along its surface, carrying a roller, the circumference of which is 12·8 inches, and turned a little conical for the sake of adjustment. The roller may be divided into 32 parts, each of which when transferred to the bar will give intervals of 0·4 of an inch each: The angle of the subdividing sector should of course be 11° 15′, and subdivided into four parts, which will divide the inch into tenths: The surface may also receive other lines, with subdivisions suited to the different purposes for which it may be wanted. The revolutions of the roller and its $\frac{1}{32}$ parts must be dotted upon the bar; taking care, by sizing the roller, to come as near the true standard measure as possible: When this is done, compare the extent of the greater disectional number that is contained in the length; *i. e.* 128 intervals or 51·2 inches, with the standard measure; noting the difference as indicated by the micrometer heads. The examination and construction of the table of errours may then be conducted just as was done for the circle.

Method of applying it.

Being now ready for the performance of its work, the scale to be divided must be laid alongside of the bar, and the true divisions must be cut upon it by an appeal, as before, to the erroneous dots on the bar, corrected by a corresponding table of errours. The apparatus, remaining entire in the possession of the workman, with its primitive dots, the table of errours, &c., is ready for dividing another standard, which will be precisely similar to others that have been, or may be, divided from it. It may be considered, indeed, as a kind of engine; and as it is not vitiated by the coarse operation of racking with a screw, but performed by only looking at the work, the method will command about three times the accuracy that can be derived from the usual straight-line dividing engine. Should it be asked, if an engine thus appointed would succeed for dividing circles? I answer, Yes; but I would not recommend it; because, beyond a certain extent of radius, it is not necessary; for the errours, which would be introduced into the work by the violence of racking a large wheel, are sufficiently reduced by the comparative shortness of the radius of such instruments as we divide by that method: And, what is still more to the purpose, the dividing engine is four times more expeditious, and bears rough usage better. I cannot quit the subject of dividing straight lines without observing, that I never had my apparatus complete. The standard which I made for Sir George Shuckburg Evelyn in 1796 was done by a mere make-shift contrivance, upon the principle of dividing by the eye; how I succeeded may be seen in Sir George's papers on Weights and Measures (Phil Trans. for 1798). I made a second, some years after, for Professor Pictet of Geneva, which became the subject of comparison with the new measure of France, before the National Institute; and their report, drawn up by Mr. Pictet, has been ably restated and corrected by Dr. Young, as published in the Journals of the Royal Institution. I made a third for the Magistrates of Aberdeen. I notice the two latter, principally to give myself an opportunity of saying, that, if those three scales were to be compared together, notwithstanding they were divided at distant periods of time, and at different seasons of the year, they would be found to agree with each other, as nearly as the different parts of the same scale agree.

Such an engine might be used for dividing circles, but not to be recommended.

Standard measures.

I hope I may here be allowed to allude to an inadvertency, which has been committed in the paper mentioned above; and which Sir George intended to have corrected, had he lived to conclude his useful endeavours to harmonize the discordant weights and measures of this country. The instruments which he has brought into comparison are, his own five feet standard measure and equatorial; General Roy's forty-two inch scale; the standard of Mr. Aubert; and that of the Royal Society. The inadvertency is this: In his equatorial, and the standard of the Royal Society, he has charged the errour of the most erroneous extent, when compared with the mean extent, alike to both divi-

Inadvertency of sir G. Shuckburg Evelyn:

sions; *i. e.* he has supposed one of the divisions which bound the erroneous extent, to be too much to the right, and the other too much to the left, and that by equal quantities: This is certainly a good-natured way of stating the errours of work; and perhaps not unjustly so, where the worst part has been selected; but, in the other three instances, namely, in General Roy's, Mr. Aubert's, and his own standard, he has charged the whole errour of the most erroneous extent to one of the bounding lines.

General accuracy of Bird's dividing. The Greenwich quadrant

I was well confirmed in my high opinion of the general accuracy of Bird's dividing, when, last winter*, I measured the chords of many arcs of the Greenwich quadrant. That instrument has indeed suffered both from a change in its figure, and from the wearing of its centre; but the graduation, considering the time when it was done, I found to be very good. Sir George in his paper upon the equatorial (Phil. Trans. for 1793), after some compliments paid to the divider of his instrument, says, "the late Mr. John Bird "seems to have admitted a probable discrepancy in the divisions of his eight feet quadrant amounting to 3″;" and he refers to Bird on the construction of the Greenwich quadrant. This quantity being three times as great as any errours that I met with, I was lately induced to inquire how the matter stood. Bird, in the paper referred to, says, "in dividing this instrument I never met with an inequality "that exceeded one second. I will suppose, that in the 90 "arch this errour lay toward the left hand, and in the 96 "arch that it lay towards the right, it will cause a difference between the two arches of two seconds; and, if an "errour of one second be allowed to the observer in reading off his observation, the whole amount is no more than "three seconds, which is agreeable to what I have heard, "&c." Sir George's examination of his own equatorial furnishes me with the means of a direct comparison: in his account of the declination circle, we find an errour + 2″·35, and another — 1″·5; to these add an errour of half a second in each, for reading off, which Sir George also admits, we shall then have a discrepancy of 4″·85; but, as the errours

compared with Sir G. S. Evelyn's equatorial.

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* This paper was written in June, 1808.

of reading off are not errours of division, let them be discharged from both, and the errours will then stand, for the quadrant 2″, and for the circle 3·″85. As the radius of the former, however, is four times greater than that of the latter, it will appear, by this mode of trial, that the equatorial is rather more than twice as accurately divided as the quadrant. In doing justice to Bird in this instance, I have only done as I would be done by: for, should any future writer set me back a century on the chronological scale of progressive improvement, I hope some one will be found to restore me to my proper niche. I now subjoin a restatement of the greatest errour of each of the instruments that are brought into comparison by Sir George, after having reduced them all by one rule; viz. allowing each of the two points which bound the most erroneous extent to divide the apparent errour equally between them. They are expressed in parts of an inch, and follow each other in the order of their accuracy.

Errours of different standards.

Sir George Shuckburg's 5 feet standard	·000165
General Roy's scale of 42 inches	·000240
Sir George's equatorial, 2 feet radius..........	·000273
The Greenwich quadrant, 8 feet radius	·000465
Mr. Aubert's standard, 5 feet long	·000700
*The Royal Society's standard, 92 inches long,	·000795

For the justness of the above statement I consider my name as pledged; requesting the permission to say, that if on the result of each respective examination, as here presented, there could have been more than one opinion, it would not have appeared here. I am farther prompted to add, that the above comparative view presents one circumstance to our notice, which cannot do less than gratify every individual, who is at all conversant in these matters; I mean, the high rank which General Roy's scale takes in the list; that scale having been made the agent in measuring the base line of our national trigonometrical survey.

Care must be taken, to have the circle of a

To return, finally, to the dividing of circles; I must state, as matter of precaution, that great care should be taken

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* This is the same which Mr. Bird used in dividing his eight feet mural quadrants, and was presented to the Royal Society by Bird's executors.

uniform temperature while turning the outer edge,

taken during the turning of the outer edge, to have the circle of the same temperature; for one part may be expanded by heat, or contracted by cold, so much more than another, as to cause the numbers in the tables of errours to be inconveniently large. A night is not more than sufficient for allowing the whole to take the same temperature, after having been handled by the workmen; and the finishing touch should be given within a short space of time. But, if the effects of temperature are to be regarded in turning a circle, it is of tenfold more importance to attend to this circumstance, while the examination of the larger arcs of the instrument is carried on; for it is absolutely necessary, that, during this time, the whole circle should be of the same heat exactly. Few workmen are sufficiently aware of this: They generally suppose the expansion of metals to be a trifle, which need not be regarded in practice; and wonder how the parts of a circle can be differently heated, without taking pains to make it so. One degree of Fahrenheit's thermometer indicates so small a portion of heat, that, in such places as workmen are usually obliged to do their business in, it is not very easy to have three thermometers attached to different parts of a large instrument, showing an equality of temperature within that quantity: Yet so necessary is correctness in this respect, that if a circle has the vertex one degree warmer than its opposite, and if this difference of temperature be regularly distributed from top to bottom, the upper semicircle will actually exceed the lower by 2″: And, if such should happen to be the case while the examination of the first dot of the third quadrant is made, the regularity of the whole operation would thereby be destroyed.

and above all while examining the larger arcs.

Effects of temperature much more important than generally supposed.

The apparatus not expensive

It may not be improper to remark, that dividing by the eye does not require a more expensive apparatus than the operation of dividing by hand; and, indeed, less so when the scale of inches is deemed necessary. The method by adjustment is still more expensive, requiring whatever tools Bird's method requires, and, in addition to these, a frame and microscopes, somewhat similar to those for dividing by the eye.

Much time

It is somewhat more difficult to give a comparative estimate saved by it

mate of the time, which the different methods of dividing require. I know, that thirteen days of eight hours each are well employed in dividing such a circle by my method; about fifty-two days would be consumed in doing the same thing by Bird's method; and I think I cannot err much, when I state the method by adjustment, supposing every dot to be tried, and that two thirds of them want adjusting, to require about one hundred and fifty of such days.

The economy of time (setting aside the decided means of accuracy) which the above estimate of its application offers to view, will, I think, be considered of no little moment. By the rising artist, who may aspire to excellence, it will at least, and I should hope, with gratitude, be felt in the abbreviation of his labours. To me, indeed, the means of effecting this became indispensible; and it has not been without a sufficient sense of its necessity, that I have been urged to the progressive improvement and completion of these means, as now described. It is but little, that a man can perform with his own hands alone; nor is it on all occasions, even in frames of firmer texture than my own, that he can decisively command their adequate, unerring, use. And I must confess, that I never could reconcile it to what I hold as due to myself, as well as to a solicitous regard for the most accurate cultivation of the science of astronomy, to commit to others an operation requiring such various and delicate attentions, as the division of my instruments.

That my attentions on this head have not failed to procure for me the notice and patronage of men, whose approbation makes, with me, no inconsiderable part of my reward, I have to reflect on with gratitude and pleasure: and as I look with confidence to the continuance of that patronage, so long as the powers of execution shall give me the inclination to solicit it, I cannot entertain a motive, which might go to extinguish the more liberal wish of pointing out to future ingenuity a shorter road to eminence; sufficiently gratified by the idea of having in the present communication contributed to facilitate the operations, and to aid the progress of art (as far as the limited powers of vision will admit) toward the point of perfection.

Table of apparent errours.

Name of the Dot.	First Quadrant.	Second Quadrant	Third Quadrant.	Fourth Quadrant.
0°·0	0	+ 12·2	— 6·9	+ 17·9
45·0	— 21·3	— 8·9	16·7	— 29·6
22·5	1·6	2·2	1·0	2·7
67·5	+ 1·0	+ 15·6	0·0	+ 13·7
11·2	— 16·6	— 20·2	22·6	— 30·3
33·7	4·0	4·2	13·2	23·1
56·2	16·9	22·2	17·0	22·7
78·7	30·8	16·6	31·3	30·3
5·6	2·7	8·6	4·1	10·1
16·9	11·5	11·3	11·2	16·1
28·1	9·0	7·4	5·8	14·3
39·4	9·3	8·2	5·8	13·1
50·6	4·2	6·6	8·2	4·4
61·9	4·3	8·4	12·5	4·4
73·1	7·6	10·0	13·6	9·7
84·4	18·0	+ 6·0	16·3	7·1
2·8	3·4	— 7·5	8·9	2·1
8·4	0·0	5·0	4·6	5·7
14·1	6·6	8·2	5·6	4·8
19·7	1·6	2·4	+ 1·0	2·5
25·3	3·7	8·2	— 2·9	2·5
30·9	+ 2·4	7·1	7·0	0·0
30·6	— 5·9	+ 1·0	2·5	1·5
42·2	+ 3·1	1·9	5·8	+ 2·5
47·8	7·1	5·2	+ 2·4	4·8
53·4	— 5·6	— 6·0	— 5·0	— 6·1
59·1	10·7	+ 1·0	3·0	+ 1·4
64·7	7·9	— 18·0	10·7	— 9·0
70·3	2·7	7·4	1·5	9·0
75·9	1·2	5·2	2·2	4·7
81·6	1·6	+ 1·7	0·0	2·0
87·2	13·7	6·0	3·5	+ 5·6

Table of apparent errours.

First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	Name of the Dot.
+ 4·6	+ 17·1	— 4·4	+ 17·3	1°·4
— 5·2	— 9·7	8·9	— 6·4	4·2
0·0	3·8	1·0	4·7	7·0
+ 1·0	+ 3·5	5·1	5·5	9·8
— 5·5	— 1·6	0·0	+ 1·2	12·7
7·6	7·6	4·2	— 2·3	15·5
9·4	3·9	0·0	5·3	18·3
+ 1·1	+ 12·1	+ 4·2	+ 4·3	21·1
12·3	0·9	6·2	14·4	23·9
— 5·7	6·2	1·1	— 14·2	26·7
+ 1·5	3·5	— 6·3	4·2	29·5
0·0	7·0	7·7	+ 1·4	32·3
1·5	+ 9·0	+ 3·0	4·3	35·2
— 8·6	— 5·9	— 2·0	— 6·7	38·0
3·3	+ 2·7	4·9	1·5	40·8
+ 4·0	3·1	3·5	+ 1·0	43·6
13·5	10·5	+ 16·0	14·9	46·4
2·1	0·0	1·7	— 3·5	49·2
— 5·0	— 10·7	— 2·9	1·5	52·0
4·2	7·9	2·2	7·2	54·8
4·0	3·0	2·5	1·0	57·7
7·3	+ 6·2	6·1	1·5	60·5
8·2	— 10·1	5·6	12·7	63·6
1·4	7·2	3·9	+ 2·2	66·1
+ 11·2	+ 14·9	+ 21·2	7·2	68·9
— 7·1	— 1·0	— 8·9	— 11·7	71·1
5·3	1·2	6·6	2·7	74·5
7·2	0·9	+ 1·0	5·9	77·3
0·5	1·8	5·3	2·6	80·2
+ 4·4	+ 1·4	— 2·2	4·3	83·0
— 20·8	— 0·0	11·4	+ 1·0	85·8
+ 2·1	+ 11·0	4·0	9·5	88·6

Table of real errours.

Nameo f the Dot.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.
0°·0	0·0	+ 8.8	— 6·9	+ 14·4
1·4	— 4·8	— 0·6	16·0	5·9
2·8	10·2	9·3	24·0	— 2·9
4·2	13·8	15·1	28·3	12·8
5·6	13·7	12·5	23·3	16·1
7·0	15·9	16·8	28·7	19·4
8·4	17·6	19·6	32·0	27·0
9·8	21·4	16·1	35·5	30·7
11·2	21·6	16·7	31·5	26·5
12·7	27·9	21·6	32·2	28·6
14·1	31·1	26·8	37·5	34·4
15·5	28·5	22·7	30·2	26·8
16·9	27·3	20·5	32·4	32·7
18·3	29·9	18·2	24·2	25·7
19·7	20·2	13·5	20·6	22·2
21·1	22·4	5·9	22·1	24·0
22·5	10·0	1·8	10·9	6·7
23·9	8·8	12·2	16·0	14·9
25·3	19·8	15·5	20·2	24·0
26·7	21·7	16·1	20·0	33·0
28·1	22·1	12·8	23·8	36·4
29·5	17·1	15·8	28·9	35·0
30·9	22·1	18·0	31·4	37·0
32·3	24·7	19·3	33·3	37·7
33·7	17·4	9·1	25·1	37·6
35·2	22·7	8·0	25·1	35·7
36·6	27·3	11·9	27·4	41·8
38·0	26·5	15·6	26·9	40·6
39·4	26·4	16·7	24·8	43·1
40·8	25·4	7·2	25·1	33·6
42·2	18·5	10·4	24·7	30·2
43·6	16·3	10·0	24·6	31·7
45·0	16·9	8·0	13·0	22·4

Table of real errours.

First Quadrant.	Second Quadrant	Third Quadrant.	Fourth Quadrant.	Name of the Dot
— 16·9	— 8·0	— 13·4	— 22·4	45°0
8·7	5·5	9·7	16·1	46·4
14·3	9·6	17·4	22·3	47·8
22·3	17·9	19·9	33·8	49·2
26·0	21 6	26·7	31·9	50·6
25·5	26·0	23·6	28·9	52·0
32·0	27·8	30·3	38·3	53·4
34·0	27·3	29 1	35·2	54·8
26·8	22·1	24·0	32·6	56·2
29·6	24·5	29 7	29·8	57·7
33·7	17·7	27·2	24·6	59·1
30·2	15 6	29·3	26·5	60·5
19·2	15·3	24 1	19.4	61·9
21·5	14·6	18·8	23·7	63·3
19·0	21·5	22·4	17·4	64·7
18·8	19·9	22·8	17·1	66·1
3·0	+ 8·2	+ 0 7	+ 2·5	67·5
9·8	— 2·8	— 2·5	— 13·0	68 9
15·7	10·2	13·7	19·2	70·3
21·9	7·0	21·8	25·8	71 7
23·0	13·9	25·1	23·0	73·1
27·1	14·3	25·3	26·8	74·5
26·6	20·1	26·6	30·7	75·9
33 3	21·1	22·7	31·1	77·3
27·9	16·0	23·8	29·1	78·7
35·5	14·5	18·5	28·7	80·2
29·3	9·0	22·4	27 3	81·6
21·0	6·6	17·5	21·4	83·0
27·5	5·4	21·0	21·6	84·4
31·0	7·9	15·4	12 6	85·8
23 0	0 1	6·8	5·2	87·1
16·3	3·7	15·9	6·4	88·6
+ 8·8	0·9	+ 14·4	0 0	90·0

VIII.

On the Origin and Formation of Roots. In a Letter from T. A. Knight, *Esq. F. R. S. to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S*[*].

My dear Sir,

Buds of trees spring from the alburnum.

In my former communication I have given an account of some experiments, which induced me to conclude, that the buds of trees invariably spring from their alburnum, to which they are always connected by central vessels of greater or less length; and in the course of much subsequent experience I have not found any reason, to change the opinion that I have there given[†]. The object of the present communication is to show, that the roots of trees are always generated by the vessels which pass from the cotyledons of the seed, and from the leaves, through the leafstalks and the bark, and that they never, under any circumstances, spring immediately from the alburnum.

The radicle in the seed not the root.

The organ, which naturalists have called the radicle in the seed, is generally supposed to be analogous to the root of the plant, and to become a perfect root during germination; and I do not know that this opinion has ever been controverted, though I believe, that, when closely investigated, it will prove to be founded in errour.

Root lengthen by new parts added at the points.

A root, in all cases with which I am acquainted, elongates only by new parts, which are successively added to its apex or point, and never, like the stem or branch, by the extension of parts previously organized; and I have endeavoured to show, in a former memoir, that, owing to this difference in the mode of the growth of the root and lengthened plumule of germinating seeds, the one must ever be obedient to gravitation, and points towards the centre of the Earth, while the other must take the opposite direction[‡]. But the radicle of germinating seeds elongates by the extension

Radicle of

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[*] From the Philos. Transact. for 1809, p. 169.

[†] Phil. Trans. 1805; Journal, vol XIII, p. 349.

[‡] Phil. Trans. 1806; Journal, vol. XIV, p. 4.

tension of parts previously organized, and in a great number of cases, which must be familiar to every person's observation, raises the cotyledons out of the mould in which the seed is placed to vegetate. The mode of growth of the radicle is therefore similar to that of the substance which occupies the spaces between the buds near the point of the succulent annual shoot, and totally different from that of the proper root of the plant, which I conceive to come first into existence during the germination of the seed, and to spring from the point of what is called the radicle. At this period, neither the radicle nor cotyledons contain any alburnum; and therefore the first root cannot originate from that substance; but the cortical vessels are then filled with sap, and apparently in full action, and through these the sap appears to descend, which gives existence to the true root.

seeds elongated by extension of organised parts.

Origin of the root.

When first emitted, the root consists only of a cellular substance, similar to that of the bark of other parts of the future tree; and within this the cortical vessels are subsequently generated in a circle, enclosing within it a small portion of the cellular substance, which forms the pith or medulla of the root. The cortical vessels soon enter on their office of generating alburnous matter; and a transverse section of the root then shows the alburnum arranged in the form of wedges round the medulla, as it is subsequently deposited on the central vessels of the succulent annual shoot, and on the surface of the alburnum of the stems and branches of older trees*.

Its formation.

If a leaf-stalk be deeply wounded, a cellular substance, similar to that of the bark and young root, is protruded from the upper lip of the wound, but never from the lower; and the leaf-stalks of many plants possess the power of emitting roots, which power cannot have resided in alburnum, for the leaf-stalk does not contain any; but vessels, similar to those of the bark and radicle, abound in it, and apparently convey the returning sap; and from these vessels, or perhaps more properly from the fluid they convey, the roots emitted by the leaf-stalk derive their existence†.

Leaf-stalks of many plants can emit roots,

though they contain no alburnum.

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* Phil. Trans. for 1801, Plate 27.

† Phil. Trans. for 1801.

If a circle of bark be taken off, roots proceed from the portion above,

If a portion of the bark of a vine, or other tree, which readily emits roots, be taken off in a circle extending round its stem, so as to intercept entirely the passage of any fluid through the bark; and any body which contains much moisture be applied, numerous roots will soon be emitted into it immediately above the decorticated space, but never immediately beneath it: and when the alburnum in the decorticated spaces has become lifeless to a considerable depth, buds are usually protruded beneath, but never immediately above it, apparently owing to the obstruction of the ascending sap. The roots which are emitted in the preceeding case, do not appear in any degree to differ from those which descend from the radicles of germinating seeds; and both apparently derive their matter from the fluid which descends through the cortical vessels.

and buds from the portion below.

Varieties of the apple tree have excrescences formed by points that would have become roots. These readily propagated by cuttings.

There are several varieties of the apple-tree, the trunks and branches of which are almost covered with rough excrescences, formed by congeries of points, which would have become roots under favourable circumstances; and such varieties are always very readily propagated by cuttings. Having thus obtained a considerable number of plants of one of these varieties, the excrescences began to form upon their stems when two years old, and mould being then applied to them in the spring, numerous roots were emitted into it early in the summer. The mould was at the same time raised round, and applied to, the stems of other trees of the same age and variety, and in every respect similar, except that the tops of the latter were cut off a short distance above the lowest excrescence, so that there were no buds or leaves from which sap could descend to generate or feed new roots; and under these circumstances no roots, but numerous buds were emitted, and these buds all sprang from the spaces and points, which under different circumstances had afforded roots. The tops of the trees last mentioned, having been divided into pieces of ten inches long, were planted as cuttings, and roots were by these emitted from the lowest excrescences beneath the soil, and buds from the uppermost of those above it.

Roots & buds seemingly convertible,

but buds proceed from the

I had anticipated the result of each of the preceding experiments; not that I supposed, or now suppose, that roots can sap carried upward by the alburnum,

roots from the sap sent downward by the bark.

can be changed into buds, or buds into roots; but I had before proved, that the organization of the alburnum is better calculated to carry the sap it contains from the root upwards, than in any other direction; and I concluded, that the sap, when arrived at the top of the cutting through the alburnum, would be there employed, as I had observed in many similar cases, in generating buds, and that these buds would be protruded where the bark was young and thin, and consequently afforded little resistance*. I had also proved the bark to be better calculated to carry the sap towards the roots than in the opposite direction, and I thence inferred, that as soon as any buds, emitted by the cuttings, afforded leaves, the sap would be conveyed from these to the lower extremity of the cuttings by the cortical vessels, and be there employed in the formation of roots.

The same fluid probably forms both root and bud.

Both the alburnum and bark of trees evidently contain their true sap; but whether the fluid, which ascends in such cases as the preceding through the alburnum to generate buds, be essentially different from that which descends down the bark to generate roots, it is perhaps impossible to decide. As nature, however, appears in the vegetable world to operate by the simplest means; and as the vegetable sap, like the animal blood, is probably filled with particles which are endued with life; were I to offer a conjecture, I am much more disposed to believe, that the same fluid, even by merely acquiring different motions, may generate different organs, than that two distinct fluids are employed to form the root, and the bud and leaf.

Roots in which there is alburnum capable of producing buds.

When alburnum is formed in the root, this organ possesses in common with the stem and branches, the power of producing buds, and of emitting fibrous roots; and when it is detached from the tree, the buds always spring near its upper end, and the roots near the opposite extremity, as in the cuttings abovementioned. The alburnum of the root is also similar to that of other parts of the tree, except that it is more porous, probably owing to the presence of abundant moisture during the period in which it is deposited†. And possibly the same cause may retain the wood of the root permanently in the state of alburnum; for I have shown, in a former memoir, that if the mould be taken away, so that the parts of the larger roots, which adjoin the trunk, be exposed to the air, such parts are subsequently found to contain much heart wood*.

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* Phil. Trans. for 1805; Journal, vol. XIII, p. 349.

† Phil. Trans. for 1801.

Buds and fibrous roots of perennial herbaceous plants produced from substances corresponding to the alburnum and bark.

I would wish the preceding observations to be considered as extending to trees only, and exclusive of the palm tribe; but I believe they are nevertheless generally applicable to perennial herbaceous plants, and that the buds and fibrous roots of these originate from substances which correspond with the alburnum and bark of trees. It is obvious, that the roots, which bulbs emit in the spring, are generated by the sap, which descends from the bulb when this retains its natural position; and such tuberous rooted plants as the potato offer rather a seeming than a real obstacle to the hypothesis I am endeavouring to establish. The buds of these are generally formed beneath the soil; but I have shown, in a former memoir, that the buds on every part of the stem may be made to generate tubers, which are similar to those usually formed beneath the soil; and I have subsequently seen, in many instances, such emitted by a reproduced bud without the calyx of a blossom, which had failed to produce fruit; but I have never, under any circumstances, been able to obtain tubers from the fibrous roots of the plant.

The tuber little different from a branch,

The tuber therefore appears to differ very little from a branch, which has dilated instead of extending itself, except that it becomes capable of retaining life during a longer period; and when I have laboured through a whole summer to counteract the natural habits of the plant, a profusion of blossoms has in many instances sprung from the buds of a tuber.

and runners of tuberous rooted plants similar in organization to the stem.

The runners also, which, according to the natural habit of the plant, give existence to the tubers beneath the soil, are very similar in organization to the stem of the plant, and readily emit leaves and become converted into perfect stems, in a few days, if the current of ascending sap be diverted into them; and the mode in which the tuber is formed above and beneath the soil is precisely the same. And when the sap, which has been deposited at rest during the autumn and winter, is again called into action to feed the buds, which elongate into parts of the stems of the future plants in the spring, fibrous roots are emitted from the the bases of these stems, whilst buds are generated at the opposite extremities, as in the cases I have mentioned respecting trees.

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* Phil. Trans. for 1801.

Fibrous roots, of bulbous and tuberous rooted plants annual;

of trees perennial.

Many naturalists* have supposed the fibrous roots of all plants to be of annual duration only, and those of bulbous and tuberous rooted plants certainly are so; as in these nature has provided a distinct reservoir for the sap which is to form the first leaves and fibrous roots of the succeeding season: but the organization of trees is very different, and the alburnum and bark of the roots and stems of these are the reservoirs of their sap during the winter†. When, however, the fibrous roots of trees are crowded together in a garden-pot, they are often found lifeless in the succeeding spring; but I have not observed the same mortality to occur in any degree, in the roots of trees when growing, under favourable circumstances, in their natural situation.

I am prepared to offer some observations on the causes which direct the roots of plants in search of proper nutriment, and which occasion the root of the same plant to assume different forms under different circumstances; but I propose to make these observations the subject of a future communication.

I am, my dear Sir,
with great respect,
your much obliged servant,
Elton, Dec. 22, 1808. THOMAS AND. KNIGHT.

IX.

Inquiries into the Limits of Single Vision, and the corresponding Points in the Retina, &c.: by Dr. HALDAT, *Secretary to the Academy of Nancy*‡.

Corresponding points on the retina.

IN the paper on double vision, printed in this Journal for november, 1806||, I referred the singular phenomena there

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* M. Mirbel's *Traité d'Anatomie*, &c. Dr. Smith's Introduction to Botany.

† Phil. Trans. for 1805.

‡ Journ. de Physique, vol. LXV, p. 16.

|| See Journal, vol. XVII, p. 201.

recited to the general laws of vision, by applying to them the theory of corresponding points in the two retinas. These points, and the limits of single vision, will form the subject of this second paper.

Some correspondence between parts of the two eyes to produce single vision.

The slightest attenion to the functions of the eyes must long have taught physiologists, that certain relations between these organs are necessary for the double impression of one object to produce a single perception. The admirable sympathy that prevails between the muscles that move the eyes, which constantly determines these organs to turn to the same point; the difficulty of fixing one without the other; and the almost absolute impossibility of moving them simultaneously in different directions; would have been sufficient to show, that these organs are formed to be affected in concert, if the injuries affecting this arrangement, and the most easy ocular demonstration did not present themselves to confirm it. Accordingly they are unanimous in acknowledging the necessity of this correspondence between the points of the retinas, which receive the impression of the double image at the same time. But in what does this correspondence consist? can it be established between points taken throughout the whole extent of the retina, between certain of these points, or between two of them alone? These are questions on which authors are not agreed, or which, not having been examined, appear to me to deserve investigation.

Haller confines the field of vision to the optic axis.

Haller, who in the 5th vol. of his Elements of Physiology has collected what had been done by most of the learned who had written on vision, has satisfied himself with laying down from Mariott's celebrated experiment the proposition, that the image cannot produce any impression when received on the optic nerve, but that it does when received on its outer side at the point where the posterior extremity of the axis of the ball of the eye, or optic axis, terminates; a part of the retina, which he says, is supposed to be most sensible.

Le Cat extends it to the whole retina.

Le Cat, perceiving the impossibility of limiting the position of the image so strictly, has given the whole of the bottom of the eye for its field. Hence it follows, that in the opinion of Haller the points of correspondence are necessarily found in the meeting of the optic axis, since this is the only point capable of transmitting the impression: in that of Le Cat on the contrary, these points may occupy different parts of the retina, which he calls the bottom of the eye. Between authorities so respectable experiments alone can decide. Those to which I have had recourse are the most simple: they consist chiefly in producing an artificial squinting, by changing the customary direction of the optic axis by mechanical means. Every one knows, that such a change, when carried to a certain degree, will produce the perception of a double image, which, being caused by a change in the respective situation of the points of the retina simultaneously affected in ordinary cases, must be calculated to elucidate what relates to the place of the image.

Experiments.

The firſt result of the experiments by means of artificial squinting is, that the phenomena it exhibits are not reconcilable with the opinion, in which the situation of the image is confined to a single spot at the bottom of each eye; for one of the impressions, being then necessarily made on a part of the retina not intended to receive it, ought not to give rise to any perception. From this single fact we may conclude, that the place of the image is not necessarily at the summit of the optic axis; but that several parts of the surface of the retina are capable like it of receiving the impression, and producing perception. But we may assure ourselves of the fact, by closing one eye, fixing the other with a speculum, and then giving a lighted candle, first placed in the line of the optic axis, various angular positions. Its flame will not cease to be visible, till the angle is at least 70°. It is true we do not see it perfectly, unless the angle be considerably less than this; but we do not cease to perceive the flame, till its position is such, that none of the direct rays can reach the posterior hemisphere of the eye. It is evident therefore, that nature has not given the retina so great an extent in vain; and that the parts lying round the optic axis, though in a less favourable situation, are equally capable of being affected. I will add, that, having compared the impressions produced by rays parallel with the optic axis, and others inclined to this axis, they appeared to me to differ in vividness only as far as would result from the diminution of the aperture of the pupil occasioned

Squinting proves, that it is not confined to a single spot.

It extends over the whole posterior hemisphere of the eye,

and objects are seen best in the centre only because most rays can reach it.

oned by the obliquity of its plane to the luminous rays, and by the obliquity of these rays to the refractive media.

All parts of the retina are probably equally sensible:

It is true the vividness of the images situate at the optic pole can be compared with that of those around it only by estimation; but this is sufficient to prove, that their difference is not such as it would be, if it were owing to a diminution of sensibility in the parts of the retina remote from the optic pole, and that consequently the retina is not deprived of the faculty of transmitting the impressions received on those parts; whence the points of correspondence are not necessarily and invariably situate in the optic axis, as several authors have supposed. I would not venture to assert, that all the parts of the retina, on which images may be painted, are equally sensible, the experiments I have made not being incontestibly decisive of this: but they prove at least, that the difference is not very great at a certain distance from the optic axis; for, notwithstanding the influence of the causes before mentioned, the light of a candle received into the two eyes, inclining them sufficiently for the images to make with the optic axis angles of 15°, 20°, and 25°, did not exhibit any diminution of lustre, that could be appreciated. These facts, which may easily be verified, give to the field of distinct vision, and to that of perfect vision, a much more considerable extent than is assigned to them by Dr. Young in a learned paper on vision, an abridgment of which is given in the 18th vol. of the *Bibliothèque Britannique*.

at least there is no great difference to some distance round the axis.

Dr. Young.

The image most perfect in the optic axis.

The point that corresponds with the optic axis, without being endued with sensibility superior to that of other parts of the retina, is the place of perfect vision, because it is in the focus of the refractive apparatus, the only point where the image can have its whole perfection. The muscles, determined by habit to dispose the ball of the eye so as to receive the most vivid impression, bring it into this position, because it is the most advantageous, and not because it is the only one in which vision can take place; which is equally proved by artificial squinting, and by that accidental squinting which Cheselden mentions.

Cheselden.

The corresponding point:

The optic poles are most commonly the place of the double image, or points of correspondence. These parts however

ever are not the only ones capable of receiving images; the space round the optic axes to a certain extent is also capable of being affected by them: these are the consequences deducible from the facts already mentioned. But as the double image produces a single perception only when it falls on corresponding points, that is to say, which agree in their functions, whatever in other respects this part of the retina may be, the question is now to determine on what parts the images received will produce single or double vision of the same object. As the muscles of the eyeballs sympathise together, so as constantly to arrange themselves in the same manner; and this is generally so as for the eyes to receive the greatest number of parallel, or nearly parallel rays; the points of correspondence are usually at the summit of the optic axis, that is in parts which are in precisely the same situation on the retina, or which are symmetrical. In this case vision is single, as most physiologists have said, because the impressions are similar, being equal, and received on similar parts. In rapid movements of objects however, in those of the eyes produced by passions of the mind, in the situation of objects within the distance of distinct vision, and in certain positions of the head, how can we conceive, that the impressions are made on parts precisely symmetrical? This simple reflection had long inclined me to conceive, that images might be received on points of the retina not symmetrical without giving rise to double vision; and experiment has fully convinced me of its truth.

commonly in the optic axes.

Vision is single when the two impressions are received on similar parts,

and sometimes when on parts not precisely similar.

If, after having placed a lighted candle at the distance of eight or ten feet, it be looked at with one eye, and, the position of this eye being then fixed with a speculum, the other be moved inwardly or outwardly by pressing on it with one of the fingers, so as to incline the optic axes to one another, we shall have a double image of the candle.

A double image produced by altering the relative position of the optic axes.

A double image is also obtained by inclining the two optic axes to one another at the same time, by turning the eyes either inwardly or outwardly, and inclining them to the rays of the candle. The parallelism of the transverse axes of the eyeballs must be carefully preserved, not to confound the effects occasioned by the inclination of the axes in the vertical plane with those owing to their inclination in the horizontal plane.

If, one of the two eyes being shut, the other be directed to some object placed above or below the candle, and, after it is fixed in this situation, the other eye be suddenly opened, we shall have a double image of the candle.

The axes must form a greater angle horizontally than vertically.

A slight inclination of the optic axes in the vertical plane gives rise to the double image; but it will not occur in the horizontal plane, unless there is a considerable inclination of the axes.

Turning the head so as to view an object very obliquely sufficient.

The double image may be obtained in the horizontal plane, without employing any violence on the eye, that can lead us to suspect a change in the figure of this organ. It is sufficient for this to look very obliquely at a conspicuous object placed level with the eyes; which may be done by turning the head while looking at it, till the muscles are unable to give the eyeballs a parallel direction, so that they are obliged to receive rays that reach points of the retina very differently situate. We may even obtain the double image in the oblique plane, by a certain inclination of the head: but in the vertical plane it can be obtained only by mechanical means.

The corresponding points admit of some variation.

These facts, confirmed by a great number of experiments repeated and varied in several ways, which it would take up too much room to mention here, not only prove, that nature has allowed a certain latitude in the law she has imposed on herself with respect to corresponding points; and that it is not absolutely neceſſary, that the points of the retina on which impressions are made should always be similarly situate; but also that there are points on the retina, which, taken at different distances from the optic axis, may produce double images or not, according to the direction in which they arrive at the eye. Thus in the transverse plane the optic axes must be greatly inclined, to give rise to the double image; while a slight inclination produces this phenomenon in the vertical plane. These general limits of the field of single vision, or of the area of corresponding points, give the elements of an ellipsis, the longer axis of which is parallel with the transverse axis of the eye, and the shorter

The field of single vision an ellipsis.

parallel with its vertical axis, while its centre corresponds with the summit of the optic axis.

Attempts to ascertain its extent.

This general determination of the form of the field of single vision appearing to me insufficient, I made some attempts, to point out its limits with more precision. The method I employed consisted in comparing the angle formed by the double image with the angle we are obliged to form by the inclination of the optic axes to produce it. I have found, that the apparent separation of the double image in the horizontal plane was at most one third of that, which must result from the inclination given to the optic axes in this direction; while in the vertical plane the separation of the images and the inclination of the axes were pretty exactly equal: whence I inferred, that the extent of the field of single vision was three times as great in the horizontal plane as in the vertical. But as I have found, that the inclination of the optic axes in the horizontal plane must be about 15°, to give rise to a double image, the distance from the centre of the aperture of the pupil, where the rays decussate each other, to the bottom of the eye, on which they are depicted, being about 12 mil. [4·721 lines Eng.] in an adult; I find, that the opening of this angle must comprise horizontally an extent of 8 mil. [3·147 lines Eng.] on the retina: and a third of this extent being taken for the limit of the field of single vision in the vertical plane, this field will be represented by an ellipsis, the longer axis of which is 3·147 lines, and the shorter 1·049 line. This however I give but as an approximation.

Perfection of the achromatic power of the eye.

These inquiries concerning the field of single vision have led me to some reflections on the perfection of the achromatic system of the eye, which I shall add here, though they are merely accessaries to my principal subject. It is well known, that Euler was led to the discovery of achromatic glasses by considering the structure of the eye. An optical instrument composed of refractive substances of considerable curvature, which receive the luminous rays on great part of their surface, yet form at their focus an image perfectly well defined, though the distance of the object and direction of the rays may be infinitely varied, exhibits an effect so different from that of ordinary instruments, that

Euler.

this great geometrician had no doubt it must be produced by the various combination of forms and densities peculiar to the humours of the eye. This theory, disputed at first by Dollond, soon found in that artist, convinced of his errour, a man sufficiently skilful to reduce it to practice; and this union of genius and skill gave rise to those fine instruments, the invention of which must be classed with those discoveries, that do most honour to the human mind. A comparison of the achromatic power of the human eye with that of those instruments is the subject of these reflections.

Dollond.

Achromatic power of the eye compared with that of instruments.

My experiments on the limits of single vision having obliged me in some cases to give the ball of the eye a great degree of obliquity with respect to the luminous rays, which I wished to introduce into it, I could not avoid being astonished at the achromatic power of the humours, so well adapted to prevent the decomposition of light, when so many different causes seem as if they must necessarily produce it. The most perfect achromatic glasses, the object glass of which should be of a focus sufficiently short to be compared with the eye, one of an inch and a quarter for example, would not bear an aperture of more than a fifth of an inch, according to Euler's calculation. Allowing the mean aperture of the pupil to be two lines [2¼ French], the eye would be superior in achromatic power to the most perfect glasses with the same aperture. But how can we avoid being astonished at the consideration, that the size of the aperture, which limits the achromatic power of instruments of art, has no perceptible effect on that of the humours of the eye? and that the opening of the pupil may be extended to three lines [3⅓ French] without altering the clearness of the image, as I satisfied myself by dilating the iris by applying an infusion of the leaves of deadly nightshade so as to expose the greater part of the anterior surface of the crystalline?

Farther proof of its superiority.

A still farther proof of the superiority of the achromatic power of the eye over that of instruments is the application of rays greatly inclined to refractive surfaces. Experience proves, that the best made object glasses, when greatly inclined to the rays of light, immediately exhibit signs of the decomposition of light by the irises they produce. It is not thus with the eye. I have given very great obliquity to the rays I admitted into it, without perceiving any colour; and if in some experiments of artificial squinting I have thought I found slight indications of the decomposition of light, I believe they must be ascribed rather to the changes produced in the form of the humours of the eye by compression, than to any other cause.

Contrary opinion maintained by Dr. Young.

These proofs of the perfection of the achromatic system of the eye. established by experiment, and agreeable to the general opinion of natural philosophers, had seemed to me secure from all doubt. I confess therefore I was not a little surprised, to find the contrary opinion maintained by Dr. Young in the paper I have quoted. This gentleman says, it has been too lightly presumed, that one of the final causes of the structure of the eye was to render it achromatic: and to this hypothesis he objects several experiments, from which he thinks we may conclude, that the eye is not adapted to unite all the elements of light in one point, as has been asserted. An opinion adopted by Euler, and embraced by all natural philosophers, should not I think be given up, till the arguments opposed to it have been duly weighed.

Jurine said to have observed colours on the borders of objects seen indistinctly.

The author saw only a penumbra.

Small bodies showed slight appearances of the decomposition of light,

The first is drawn from the experiments of Jurine on indistinct vision. He is said, to have observed colours on the borders of objects seen indistinctly. It is true in his Treatise on distinct and indistinct Vision, printed in Smith's Optics, he speaks of the penumbra, that surrounds objects seen indistinctly, but I have looked in vain for the observation of the irises in question. To satisfy myself of their existence, I examined by daylight, and nearer than the focal distance of my eye, different bodies, and surfaces of different colours. With the coloured surfaces I constantly observed the penumbra, or circle of dissipation, as Jurine calls them, round these surfaces. This penumbra always appeared to me to be formed of the colour of the surface growing fainter from the centre to the circumference, but I did not observe an iris round any of them. Things of little bulk, as small shot, slender wires of different metals, little bits of sealingwax, resin, marble, wood, &c., observed with due precautions, and at the least possible distance as but these no proof, as they may arise from other causes than the defect of the eye.

12 or 18 lines, it is true exhibited some slight appearances of the decomposition of light; but these appearances, being but little perceptible, and in many instances doubtful, I conceive are but little calculated to weaken the supposition of the perfection of the achromatic system of the eye. In the first place, because these irises may be confounded with the penumbras that accompany all small bodies seen indistinctly, or with the shadows themselves which are coloured in certain circumstances: but more especially because the faint irises observed may be produced, not by the unequally refracting power of the humours of the eye with respect to the various rays, but by the attraction of these bodies for the luminous rays, which, being reflected by the planes on which they infringe, graze their surface before they come to the eye; and this appears to me the more evident, because, when placed on a black ground, which cannot reflect any luminous rays, these irises entirely disappear. The account of this phenomenon, which I shall give below, I hope will leave no doubt on this theory of the irises, that accompany small bodies seen at a very short distance. These experiments then are so far from affording proofs of the imperfection of the achromatic system of the eye, that I deduce from them arguments for its great perfection.

Penumbra of small objects seen indistinctly not caused by the refractive powers of the eye.

The penumbra of small objects seen indistinctly, its extent, and the irregularity of its colour, depend on the unequal refraction of the luminous rays, that arrive at the transparent cornea with different degrees of inclination, and a diverging direction. It is evident, that, if the heterogeneous rays be unequally refracted by the humours of the eye, spots of different colours, seen at equal distances, should exhibit penumbras unequal in extent, and proportional to the refractive powers of each kind of rays they reflect, Now this is contradicted by experience. Little circular pasteboards, perfectly equal in size and three millimeters [1·18 line] in diameter, painted red, yellow, blue, and green, seen on black and white grounds, at equal distances, and at one time, constantly exhibited penumbras equal in extent.

Experiments with the opto-

The second argument opposed to the theory of the perfection of the achromatic system of the eye is derived from experiments meter urged by Dr. Young.

experiments made with the optometer. This is the name Dr. Young gives to an instrument intended to find the focal distance of the eye, or focus of distinct vision. The instrument is composed principally of a thin slip of some substance with two very small apertures, through which objects are to be looked at in a certain position. The irises that exhibit themselves at the surface of bodies seen through these small apertures are considered by the learned Englishman as produced by the unequally refractive power of the humours of the eye on different luminous rays. However, to substantiate this inference it should have been demonstrated, that the decomposition of the light cannot proceed from any other cause: yet here the cause is evidently external to the eye, and independent of its action. Since the time of Grimaldi all natural philosophers have acknowledged the action of bodies on the luminous rays, that graze or approach their surfaces; and Newton has shown by experiments of the greatest accuracy, not only that light is attracted by bodies, and inclines toward their surfaces, but also that this attractive power of bodies acts with a different force on the different rays. Now in the present instance we look at bodies through small apertures; so that the luminous rays, by means of which we see them, necessarily experience the action of the edges of the apertures they pass through, and must thus be decomposed.

In these the light decomposed by causes independent of the eye.

Experiments to prove this.

To convince ourselves, that the irises are produced by this cause, it is sufficient to look at a distant object, the frame of a window for instance, at the same time intercepting by means of a cord, the edge of a knife, or some other thin substance, part of the rays that would arrive at the opening of the pupil; when we shall perceive the object terminated by an iris, and this iris changing its position, according as the edge of the interposed substance is vertical or horizontal, inward or outward, &c. And what other cause can be assigned for this phenomenon? Why should the eye decompose the rays, that reach it through small apertures, and not decompose those that reach it without such intervention? This explanation of irises observed at the surfaces of objects seen through small apertures is equally applicable to the faint irises observed in objects viewed indistinctly. The rays that graze their edges are decomposed; and this decomposition, which is not perceived at a distance on account of the mixture of the very numerous unaltered rays, is distinguished when very close, because they reach the retina almost without mixture.

Dr. Young's experiment of a triangular spectrum from a luminous point,

Dr. Young likewise opposes the following experiment to the theory of the perfection of the achromatic system of the eye. Having received on a prism the rays issuing from a luminous point, he observed, that, instead of obtaining a linear spectrum, as the theory of the equal refrangibility of the different rays by the humours of the eye would lead us to expect, the spectrum appeared of a triangular figure; so that, if the eye adapted itself so as to unite the red rays in one point, the blue would be refracted, and vice versa. From this fact he infers the unequal refrangibility of the different luminous rays by the humours of the eye. This experiment of Dr. Young I have repeated with great care, and on different kinds of luminous points, as on the light of a candle seen at 4 met. [13 feet] distance through an aperture 5 mil. [near 2 lines] in diameter, that of the public lanterns from 400 to 600 [440 to 660 yards] distance, and that of the fixed stars in a clear night. I observed the triangular figure mentioned by that gentleman, whenever I gave a little magnitude to the spectrum; but in proportion as I increased its dimensions in length, by giving a greater inclination to the refracting surfaces of the prism, this figure diminished, and the spectrum approached more nearly to a parallelogram, the middle of which however continued narrower than the extremities. The end occasioned by the blue rays appeared to me generally broader than that of the red; but what particularly surprised me in this experiment was the figure of a double fan, the radii of which crossed each other in the centre, taking their course apparently in different planes.

repeated by the author on points at very different distances,

the figure of the spectrum varied by changing the inclination of the prism.

The experiment not decisive.

Though this experiment seems to indicate an unequal power of refracting the different luminous rays by the humours of the eye, it is not absolutely such as the author describes it, and a fact so complicated seems to me little adapted to elucidate this theory. To conclude that the humours of the eye are not perfectly achromatic, the luminous rays should be applied to that organ as they come from the luminous body, so that they may not undergo any decomposition before they reach it, and prove that the decomposition has been effected in their passage through the humours of this organ. Now this is the case in neither of the experiments quoted: in the former the decomposition is produced by the attractive power of the edges by which the rays pass; in the latter the heterogeneous rays, isolated by the prism separately applied, may seem to be unequally refrangible, because the spectrum ceases to be linear throughout its whole extent. But on account of this extent they arrive at the transparent cornea at different angles, in proportion to these they are unequal, and they reach parts of a refracting medium, the curvature and density of which are unequal: can we then expect them to be equally refracted? The eye is intended to preserve the natural mixture of the elementary rays applied to it: to pretend, that it should recompose those, the composition of which has been altered by foreign causes, would be to require of this organ what it cannot accomplish.

The eye a perfect achromatic instrument,

The perfection of the achromatic system of the eye appears to me therefore demonstrated; and this perfection can result only from an accurate proportion between the curvatures of the solid or fluid lenses and menisci that compose it, and the refractive power of these substances, there can be no doubt, that a more accurate imitation of the structure of their organ would give us still more perfect instruments. The labours of Mr. Rochon, and those of Dr. Robert Blair, who by an ingenious combination of glass menisci and fluids enclosed in them produced instruments perfectly achromatic, afford us the moſt promising hopes of success in the application of these principles to the construction of large instruments. Mr. Chenevix has made known to us the chemical nature of the humours of the eye. Let a new Petit and another Zinn unite their efforts with a rival of Euler, to determine with the greatest accuracy the structure, figure, density, and refractive and dispersive powers of each of the humours of the eye, and by their assistance our modern Dollonds will still farther improve the achromatic telescope.

and should lead to the farther improvement of telescopes.

X.

X.

New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian Lecture for 1808. *By* HUMPHRY DAVY, *Esq. Sec. R. S. Prof. Chem. R. I*[*].

1. *Farther Inquiries on the Action of Potassium on Ammonia and on the Analysis of Ammonia.*

Action of potassium on ammonia.

THE most remarkable circumstances occuring in the action of potassium upon ammonia are the disappearance of a certain quantity of nitrogen, and the conversion of a part of the potassium into potash.

What is the gas evolved?

The first query which I advanced in the last Bakerian Lecture, on this obscure and difficult subject, was whether the gas developed in the first part of the process of the absorption of ammonia by potassium is hidrogen, or a new species of inflammable aeriform substance, the basis of nitrogen?

Hidrogen.

Experiments made to determine this point have proved, as I expected, that the gas differs in no respect from that given out during the solution of zinc in sulphuric acid; or that produced during the action of potassium on water. By slow combustion with oxigen, it generates pure water only, and its weight, in a case in which it was mixed with atmospherical air, precisely corresponded with that of an equal quantity of hidrogen.

Has nitrogen a metallic basis?

Another query which I put is, has nitrogen a metallic basis, which alloys with the metals employed in the experiment?

Probably not.

This query I cannot answer in so distinct a manner; but such results as I have been able to obtain are negative.

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[*] The account of the principal facts respecting the action of potassium on ammonia, in this communication, were read before the Royal Society, February 2, 1809. The paper was ordered to be printed March 16, 1809. At that time, having stated to the Council that I had since made some new experiments on this matter, and on the subjects discussed in the Bakerian Lecture for 1808, I received permission to add them to the detail of the former observations for publication.

Philos. Trans. for 1809, p. 450.

Nature of potassium not altered by it.

I have examined the potassium generated in the process. It has precisely the same properties as potassium produced in the common experiment of the gun-barrel; and gives the same results by combination in oxigen, and by the action of water.

Other negative proofs.

In cases in which I had distilled the olive-coloured fusible substance in an iron tray, the surface of the tray appeared much corroded, the metal was brittle, and appeared crystallized. I made a solution of it in muriatic acid; but hidrogen alone was evolved.

I distilled a quantity of the fusible substance from 9 grains of potassium in an iron vessel, which communicated with a receiver containing about 100 grains of mercury, and by a narrow glass tube the gas generated was made to pass through the mercury; the object of this process was to detect if any of the same substance, as that existing in the amalgam from ammonia, was formed; but during the whole period of distillation, the mercury remained unaltered in its appearance, and did not effervesce in the slightest degree when thrown into water.

Nitrogen converted into oxigen and hidrogen.

That the nitrogen which disappears in this experiment is absolutely converted into oxigen and hidrogen, and that its elements are capable of being furnished from water, is a conclusion of such importance, and so unsupported by the general order of chemical facts, that it ought not to be admitted, except upon the most rigid and evident experimental proofs.

The absorption of ammonia by potassium often repeated.

I have repeated the experiment of the absorption of ammonia by potassium in trays of platina or iron, and its distillation in tubes of iron more than twenty times, and often in the presence of some of the most distinguished chemists in this country, from whose acuteness of observation, I hoped no source of errour could escape.

Results of the same kind.

The results, though not perfectly uniform, have all been of the same kind as those described in page 55*. Six grains of potassium, the quantity constantly used, always caused the disappearance of from 10 to 12·5 cubical inches of well dried ammonia. From 5·5 to 6 cubical inches of hidrogen were produced, a quantity always inferior to that evolved by the action of an equal portion of the metal upon water. In the distillation from 11 to 17 cubical inches of elastic fluid were evolved, and from 1·5 to 2·5 grains of potassium regenerated.

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* Journal, vol. XXIII, p. 254.

Least ammonia where least water.

The quantity of ammonia in the products varied from a portion that was scarcely perceptible to one twelfth or one thirteenth of the whole volume of elastic fluid: and it was least in those cases in which the absence of moisture was most perfectly guarded against. Under these circumstances likewise more potassium was revived; and the unabsorbable elastic fluid, and particularly the hidrogen in smaller proportion.

Most nitrogen at first.

When the products of distillation were collected at different periods, it was uniformly found that the proportion of nitrogen to the hidrogen diminished as the process advanced.

The first portions contained considerably more nitrogen in proportion, than the gasses evolved during the electrization of ammonia, and the last portions less.

Experiment.

I shall give the results of an experiment, in which the gasses produced in distillation were collected in four different vessels, and in which every precaution was taken to avoid sources of inaccuracy.

The barometer was at 29·8, thermometer 65° Fahrenheit.

Compound of potassium and ammonia heated,

6 grains of potassium absorbed 12 cubic inches of well dried ammonia. The metal was heated in a tray of platina, and the gas contained in a retort of plate glass.

hidrogen produced.

5·8 cubical inches of hidrogen were produced.

Fusible substance distilled.

The fusible substance was distilled in an iron tube of the capacity of 3 cubical inches and a half filled with hidrogen, the adaptors connected with the murcurial apparatus contained ·8 of common air.

1st portion of gas.

The first portion of gas collected (the heat being very slowly raised, and long before it had rendered the vessel red), equalled 7·5 cubical inches. It contained ·6 of ammonia, 7 of the residuum detonated with $4\frac{1}{2}$ of oxigen gas left a residuum of 4.

2d.

The second portion, equal to 3 cubical inches, contained no ammonia. 7·2 measures of it, detonated with 3·8 of oxigen, left a residuum of 3·5.

3d.

The third portion was equal to 5 cubical inches? at this time the tube was white hot; it contained no ammonia, 8·5 of it detonated with 4·5 of oxigen diminished to 2·5.

4th.

The last portion was a cubical inch and half, collected when the heat was most intense. 4·5 measures, with 3·75 of oxigen, left a residuum of 2·8.

Contents of the tube.

The iron tube contained, after the experiment, (as was ascertained by admitting hidrogen when it was cool), 2·7 of gas; which seemed of the same composition as the last portion. The adaptors must have contained ·8 of a similar gas.

The tube contained potash in its lowest part, and its upper part potassium, which gave by its action upon water 1¾ cubical inch of hidrogen.

The fusible substance distilled with a fresh quantity of potassium.

As the largest quantity of hidrogen is always produced at that period of the process, in which the potassium must be conceived to be regenerated, and in which the gasses being in the nascent state, its power of action upon them would be greatest, it occured to me, that if nitrogen was decomposed in the operation, there would probably be a larger quantity of it destroyed by the distillation of the fusible substance, with a fresh quantity of potassium, than by the distillation of it in its common state. On this idea I made several experiments; the results did not differ much from each other, and were such as I had expected. I shall describe one process made with the same apparatus as that which I have just detailed.—Barometer was at 29·5, thermometer 70° Fahrenheit.

6 grains of potassium were employed in an iron tray; 10 cubical inches of ammonia were absorbed, a small globule of metal remained unconverted into the fusible substance. A fresh piece of potassium, weighing six grains, was introduced into the tray.

The iron tube and adaptors (having together a capacity equal to 4·3 cubical inches) contained common air.

The gas was collected in three portions, there was no absorbable quantiy of ammonia in either of them.

1st portion of gas.

The first portion, that produced before the tube became red, was eight cubical inches. 10·25 of it detonated with 3·5 of oxigen diminished to 0·8.

2nd. The second portion equalled five cubical inches; 9¼ of this, with five of oxigen, left a residuum of 3¼.

3rd. Of the third portion, 2 cubical inches and ¼ came over. 9 of it, detonated with 5 of oxigen gas, left a residuum of 1·4.

The iron tube and the adaptors contained, at the end of the experiment, as was proved by cooling and the admission of hidrogen, 2·3 cubical inches of gas, which appeared of the same composition as the third portion. Nearly 7 grains of potassium were recovered.

Results. A comparison of these results, with those stated in the preceding page, will fully prove, that there is a much smaller proportion of nitrogen to the hidrogen, in the case in which the olive-coloured substance is distilled with potassium than in the other case, and there is likewise a larger quantity of potassium converted into potash.

The loss of nitrogen, and the addition of oxigen to the potassium, are sufficiently distinct in both processes; and the want of a correspondence between these results, and those of the experiment detailed in page 65*, are not greater than might be expected, when all the circumstances of the operation are considered. In the instance, in which a double quantity of potassium was employed, more potash must have been formed from the oxigen of the common air in the tubes; and the fusible substance, in passing through the atmosphere, absorbs in different cases different quantities of oxigen and of moisture; during the intervals of the removal of the different portions of gas likewise, some globules are lost,

Effects of more rapidly raising the temperature. In instances when the heat has been more rapidly raised, I have generally found more potassium destroyed, and less nitrogen in proportion in the aeriform products. In such cases likewise, the loss of weight has been much greater; the gasses have been always clouded, and the adaptors, after being exposed to a moist air, emitted a smell of ammonia; from which it seems likely, that small quantities of the dark

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* Journal, vol. XXIII, p. 253.

gray

I am making preparations for performing the experiment in a bored tube made from a single piece of platina, and likewise in tubes made of other metals; and I hope to be able, in a short time, to have the honour of laying the results before the Society.

Different metallic tubes will be employed,

I shall make no apology for bringing forward the investigation in its present imperfect state, except by stating, that my motive for so doing is the desire of being assisted or corrected by the opinions and advice of the learned chemical philosophers belonging to this illustrious body. In an investigation connected with almost all the theoretical arrangements of chemistry, and in operations of so much delicacy, it will, I conceive, be allowed, that it is scarcely possible to proceed with too much caution, or to multiply facts to too great an extent.

The different phenomena presented by the processes of distillation in different metallic tubes may lead to new explanations of this intricate subject, and though the facts cannot be easily accounted for, except on the supposition that nitrogen is an oxide, yet till the proportions and weights are distinctly ascertained, the inquiry cannot be considered as far advanced; for in an experiment, in which the processes are so complicated and delicate, and in which the data are so numerous, it is not easy to be satisfied, that every source of errour has been avoided, and that every circumstance has been examined and reasoned upon.

which may present different phenomena.

All conclusions on the action of potassium on ammonia are immediately dependent upon the results of the electrical analysis of the volatile alkali. In a letter, which I received in the course of the last month from Dr. Henry, that excellent chemist, has stated, that he conceives I have rather under-rated the quantity of nitrogen in ammonia, according to the proportions given in the Bakerian Lecture for 1807. This notice has induced me to repeat the experiment, under new circumstances, and I find not the slightest reason for doubting of the entire accuracy of my former results.

Dr. Henry.

In the new trial, I used mercury which had been recently boiled in the tube for electrization; the ammonia was introduced after being long dried by caustic potash, from a receiver

Fresh analysis of ammonia

confirmed the former.

receiver in which it had not been generated, and which had likewise been inverted over boiling mercury. The gas left no perceptible residuum, when absorbed by water deprived of air boiling. In this process, 15 measures of ammonia expanded, so as to fill 27 measures; and the hidrogen by detonation with oxigen, over water freed as much as possible from air, proved to be to the nitrogen as 73·8 to 26·2. In the experiment three explosions were made, the oxigen being deficient in the first two; so that no nitrogen could have been condensed in the form of nitric acid.

Except when precautions of this kind are employed, as I have before noticed, no accurate data can be obtained respecting the proportions of permanent gasses obtained from ammonia by electricity.

Cautions.

When the gas is generated and decomposed over the same mercury, there is always a greater expansion than the true one; and when the mercury is not boiled in the tube, and when common water is used, the nitrogen will be always overrated, unless this errour is counteracted by an opposite errour, that of detonating with an excess of oxigen*.

Water supposed to be formed in the decomposition of ammonia.

Dr. Henry had the kindness to send me the apparatus, in which he conceived, at that time, that he had witnessed the formation of water in the decomposition of ammonia by electricity, by his ingenious method of applying hygrometrical tests.

Appearance of moisture not a decisive proof of this.

I tried one experiment only with it, and in this there seemed to me to be more moisture exhibited in the elastic matter after electrization than before, when it was cooled by the evaporation of ether: but on maturely considering this question, I do not think, that the appearance of moisture even offers a decided proof of the existence of loosely combined oxigen in ammonia. To common hygrometrical tests, water must be less sensible in ammonia than in hidrogen or nitrogen, from its tendency to be precipitated in the form of alkaline solution, and likewise probably from its having a stronger adherence to the gas; and the elastic fluid generated, from the increase of volume will be capable of containing more aqueous vapour.

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* It will be seen by Dr. Henry's letter, which immediately precedes this communication, that in repeating his processes, since this paper was written, he has gained results almost precisely the same as those indicated in the text; and there is every reason to believe, that 100 of ammonia in volume uniformly become 180, when decomposed by electricity, and that the gas produced consists in 100 parts of 74 hidrogen and 26 nitrogen. See Journal, vol. XXIV, p. 358.

It is not easy to determine, with perfect precision, the specific gravity of a gas, so light as hidrogen and even ammonia; but the loss of weight, which appears to take place in the electrical analysis of ammonia, cannot, I think, with propriety, be referred entirely to this circumstance; whether the solution that I have ventured to give* be the true one, I shall not, in the present state of the enquiry, attempt to discuss.

Hidrogen and nitrogen compounds of ammonium.

The question of ammonia being analogous to other salifiable bases in its constitution, is determined by the phenomena presented by the amalgam from that alkali; and if the conversion of nitrogen into oxigen and hidrogen should be established, it would appear that both hidrogen and nitrogen must be different combinations of ammonium with oxigen, or with water.

II. *Farther Inquiries respecting Sulphur and Phosphorus.*

Sulphur and phosphorus contain hidrogen and oxigen.

I have stated, in the last Bakerian Lecture, that hidrogen is produced from sulphur and phosphorus in such quantities, by Voltaic electricity, that it cannot well be considered as an accidental ingredient in these bodies†. I have likewise stated, that when potassium is made to act upon them, the sulphurets and phosphurets evolve less hidrogen in the form of compound inflammable gas by the action of an acid, than the same quantity of potassium in an uncombined state, and from this circumstance, I have ventured to infer, that they may contain oxigen.

Inference.

On the idea, that sulphur and phosphorus are deprived of some of their oxigen by potassium, it would follow, that when the compounds formed in this experiment are decomposed, these substances ought to be found in a new state;

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* Bakerian Lecture, 1807, p. 40; or Journal, vol. XX, p. 329.

† See Journal, vol. XXIII, p. 321 and following.

deoxigenated, as far as is compatible with their existence in contact with water.

Sulphurets and phosphurets of potassium treated with muriatic acid.

With the view of examining the nature of the substances, separated by the action of muriatic acid upon the sulphurets and phosphurets of potassium, I combined a few grains of sulphur and phosphorus with one fourth of their weight of potassium, and exposed the compounds to the action of a strong solution of muriatic acid. As in the former cases, less inflammable gas was produced than would have been afforded by equal quantities of the uncombined potassium, and considerable quantities of solid matter separated from both compounds, which after being washed, were collected in a filter.

Residuum of the sulphuret,

The substance which separated from the sulphuret, was of a dark gray colour*, and was harsh to the touch; it had no taste, and at common temperatures no smell; but when heated, it emitted the peculiar odour of sulphur. Its specific gravity was rather less than that of sulphur. It softened at a low heat, so as to be moulded like wax between the fingers. It was a nonconductor of electricity. When heated upon a surface of glass, it soon fused, entered into ebullition, took fire, and burnt with the same light blue flame as sulphur. A small particle of it, made to combine with silver, presented the same phenomena as sulphur.

and of the phosphuret.

The substance from the phosphuret was of an amber colour, and opaque. It could not be examined in the air, in the form in which it was collected (that of a loose powder), for as soon as it was wiped dry, it took fire, and burnt in the same manner as phosphorus; when melted under naphtha, it was found to differ from phosphorus, in being much deeper coloured, perfectly opaque, and very brittle. Its fusibility was nearly the same, and, like common phosphorus, it was perfectly nonconducting.

The sulphuret and phosphuret cannot be made in large quantities.

In experiments upon the union of potassium with sulphur and phosphorus the heat is so intense, that when larger quantities than a few grains are used, the glass tubes are uniformly fused or broken in pieces, and in consequence I have not been able to operate upon such a scale, as to make an accurate examination of the substances just described, and to determine the quantity of oxigen they absorb in being converted into acid. Metallic vessels of course cannot be employed; but I intend to try tubes of porcelain, in a farther investigation of the subject.

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* Possibly this colour may have been produced by the decomposition of a film of soap of naphtha adhering to the potassium.

The sulphur and phosphorus appear to have left some oxigen.

It is evident, that the sulphur and phosphorus, separated in these processes, are not in their common state; and the phenomena would certainly incline one to believe, that they are less oxigenated. It may, I know, be said, that it is possible that they are merely combined with more hidrogen, and that the sulphur in this state is analogous to the hidrogenated sulphur of Berthollet, and to the alcohol of sulphur of Lampadius.

Action of sulphur on charcoal.

But when I decompounded dry sulphuret of potash by muriatic acid, of the same kind as had been used for decompounding the sulphuret of potassium, the ſubstance produced ſeemed to be merely in that form, in which, according to the able researches of Dr. Thompson, it is combined with water; and notwithstanding the ingenious experiments of Mr. A. Berthollet, and Mr. Robiquet*, the nature of the substance produced during the passage of sulphur over ignited charcoal is far from being fully ascertained. In a series of experiments, which my brother, Mr. John Davy, had the goodness to undertake, at my request, in the laboratory of the Royal Institution, on the action of sulphur on charcoal, the products were found to be very different, according as the charcoal employed differed in its nature. In an instance, in which imperfectly made charcoal was employed, the liquor that passed over left by combustion a residuum that had all the properties of carbonaceous matter, which agrees with the observations of Messrs. Desormes and Clement; but when the charcoal had been well burnt, there was no such residuum produced. It was found, that the same charcoal might be employed in a number of processes, till it was nearly entirely consumed, and that the sulphur, not rendered liquid, might be used for several operations.

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* *Annales de Chimie*, Fev. 1807, p. 127, 145: or Journal, vol. XVIII, p. 43, 50.

In all cases mixtures of[*] sulphuretted hidrogen gas and hidrocarbonate were evolved.

Liquor obtained from charcoal often used.

I particularly examined a specimen of the liquor, which had been obtained in the last process from charcoal that had been often used. It was a nonconductor of electricity, and, when the Voltaic spark was taken in it, did not evolve gas with more rapidity than sulphur; and this gas proved to be sulphuretted hidrogen.

Absorbed muriatic acid, depositing crystals of sulphur.

Supposing the liquor to contain hidrogen in considerable quantities, I conceived that it must be decomposed by oximuriatic acid; but it merely absorbed this substance, depositing crystals of common sulphur, and becoming a fluid similar to the sulphuretted muriatic acid; though when water was introduced, hydrated sulphur was instantly formed, and muriatic acid gas evolved.

The sulphur in it contains less oxigen than in its common state.

From the quantity of carbonic acid formed by the combustion of the carburetted inflammable gas, produced in the operation of the action of well burnt charcoal upon sulphur, it may be conceived to contain oxigen. This circumstance, and the fact that no hidrate of sulphur or muriatic acid gas is formed by the operation of oximuriatic acid upon the liquor, but common sulphur precipitated; are in favour of the opinion, that the sulphur in this liquor contains less oxigen than in its common state. This idea has likewise occurred to Dr. Marcet, who is engaged in some experiments on the subject, and from whose skill and accuracy farther elucidations of it may be expected.

III. *Farther inquiries respecting carbonaceous matter.*

Charcoal exposed to the action of nitrogen.

On the idea which I have stated, page 74[†], that the diamond may consist of the carbonaceous matter combined with a little oxigen, I exposed charcoal intensely ignited, by Voltaic electricity[‡], to nitrogen, conceiving it possible, that if this body was an oxide, containing oxigen very intimately combined, it might part with it in small proportions to carbonaceous matter, and give an important result.

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[*] Five measures of the mixed gas, agitated with solution of potash, left a residuum of 3·5. These were detonated with 5·5 of oxigen; the whole diminution, was to 6. Of this residuum 2·5 appeared to be carbonic acid.

[†] See Journal, vol. XXIII, p. 333.

[‡] The apparatus was the same as that referred to page 59. [Journal, vol XXIII, p. 321.] The power employed was that of the battery of 500 belonging to the Royal Institution.

Its visible properties not changed.

The charcoal, which had been made with great care, was preserved for a quarter of an hour in a state of ignition, in which platina instantly fused. It did not appear to change in its visible properties; but a small quantity of black sublimate, which proved to be nothing more than finely divided carbonaceous matter, collected in an arborescent state upon the platina wire, to which the charcoal was attached. The gas had increased in volume one sixth; but this was owing to the evolution of carburetted inflammable gas from the charcoal, the nitrogen was unchanged in quantity, and, as far as my examination could go, in quality. The points of the charcoal, where the heat had been intense, were rather harder than before the experiment.

Charcoal kept intensely ignited in contact with oximuriatic acid gas near an hour.

I have mentioned, page 102*, that charcoal, even when strongly ignited, is incapable of decomposing corrosive sublimate. When charcoal, in a state of ignition, is brought in contact with oximuriatic acid gas, the combustion instantly ceases. I electrified two pieces of charcoal in a globe filled with oximuriatic acid gas, which had been introduced after exhaustion of the globe. They were preserved, for nearly an hour, in intense ignition, by the same means that had been employed in the experiment on nitrogen. At first, white fumes arose, probably principally from the formation of common muriatic acid gas, by the action of the hidrogen of the charcoal upon the oximuriatic acid, and the combination of the gas so produced with aqueous vapour in the globe; but this effect soon ceased. At the end of the process, the oximuriatic acid gas was found unaltered in its properties, and copper leaf burnt in it with a vivid light. The charcoal did not perceptibly differ from the charcoal that had been exposed to nitrogen. My view in making this experiment was to ascertain, whether some new combination of carbonaceous matter with oxigen might not be formed in the process; and I hoped likewise to be able to free charcoal entirely from combined hidrogen, and from

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[*] Journal, vol. XXIV, p. 104.

alkaline and earthy matter, supposing they existed in it not fully combined with oxigen. That hidrogen must have separated in the experiment, it is not possible to doubt, and on evaporating the deposit on the sides of the globe, which was in very minute quantity, and acted like concentrated muriatic acid, it left a perceptible saline residuum*.

IV. *Farther inquiries respecting muriatic acid.*

Difference between muriatic acid and oximuriatic acid.

The experiments on muriatic acid, which I have already had the honour of laying before the Society, show, that the ideas which had been formerly entertained respecting the difference between the muriatic acid and the oximuriatic acid are not correct. They prove, that muriatic acid gas is a compound of a substance, which as yet has never been procured in an uncombined state, and from one third to one fourth of water, and that oximuriatic acid is composed of the same substance, (free from water) united to oxigen. They likewise prove, that when bodies are oxidated in muriatic-acid gas, it is by a decomposition of the water contained in that substance; and when they are oxidated in muriatic acid, it is by combination with the oxigen in that body, and in both cases there is always a union of the peculiar unknown substance, the dry muriatic acid, with the oxidated body.

Strong and extensive affinities of muriatic acid.

Of all known substances belonging to the class of acids, the dry muriatic acid is that which seems to possess the strongest and most extensive powers of combination. It unites with all acid matters, that have been experimented upon, except carbonic acid; and with all oxides (including water), and all inflammable substances that have been tried, except those which appear to be elementary, carbonaceous matter and the metals; and should its basis ever be separated in the pure form, it will probably be one of the most powerful agents in chemistry.

Not obtainable

I have lately made several new attempts to procure uncombined dry muriatic acid; but they have been all unsuccessful.

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* Charcoal, over which sulphur has been passed, as in the experiments, page 465, as has been shown by Mr. A. Berthollet, contains sulphur, and this I find after being heated to whiteness; such charcoal is a conductor of electricity, and does not differ in its external properties from common charcoal.

dry and uncombined.

Silex heated with muriate of soda.

I heated intensely, in an iron tube, silex in a very minute state of division, and muriate of soda that had been fused; but there was not the smallest quantity of gas evolved. In this case, the silex had been ignited to whiteness before it was used; but when silex in its common state was employed, or when aqueous vapour was passed over a mixture of dry silex and dry salt in a porcelain tube, muriatic acid gas was developed with great rapidity.

Oxide of boracium burned in oximuriatic acid.

I have stated, page 79*, that a sublimate is formed by the combustion of the olive coloured oxide of boracium in oximuriatic acid. On the idea that this might be boracic acid, and that dry muriatic acid might be separated in the process, I examined the circumstances of the experiment; but I found the sublimate to be a compound of boracic and muriatic acid, similar to the compound of muriatic and phosphoric acid.

Muriate of ammonia heated with potassium.

I heated freshly sublimed muriate of ammonia with potassium; when the quantities were equal, as much hidrogen gas was developed as is generated by the action of water on potassium; much ammonia was evolved, and muriate of potash formed; when the potassium was to the muriate as 4 to 1, less hidrogen appeared, and a triple compound of muriatic acid, ammonia, and potassium, or its protoxide was formed, which was of a dark gray colour, and gave ammonia and muriate of potash by the action of water. There was not the slightest indications of the decomposition of the acid in the experiment. The process, in which this decomposition may be most reasonably conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions.

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* Journal, vol. XXIV, p. 15, 16.

XI.

XI.

Notes to Dr. HENRY'S *Paper on Ammonia. By the Author. Communicated in a Letter to the Editor**.

Slowness with which ammoniacal gas yields its water to deliquescent salts not a proof of strong affinity.

(A.) THE fact respecting the slowness, with which ammoniacal gas gives up its water to deliquescent salts, stated at page 360, vol. XXIV, of the Journal, may be explained, I believe, without ascribing it to any remarkably strong affinity of the gas for water. When sticks of recently fused potash are exposed to ammonia over mercury, they soon become covered with a white incrustation, which appears to prevent the potash beneath from acting on the gas, or rather on the water contained in it. It is the interposition of this substance, which most probably occasions the slow removal of the water. The desication, I aftewards found, may be effected more rapidly by introducing at intervals, fresh quantities of potash, before it has completely cooled from a state of fusion,

Best dried by small portions introduced frequently.

Gay Lussac finds few gasses contain water.

In the second volume of the *Mémoires d'Arcueil*, published at Paris during the present year (1809), and received by Mr. Dalton from the authors since the printing the foregoing paper, Gay Lussac has made, incidentally some observations on the water contained in gasses. Fluoric acid and perhaps ammoniacal gasses, he remarks, are free from water of any kind; muriatic acid from all but combined water, the proportion of which, according to his experiments, does not much differ from the determination of Mr. Davy; and the remaining gasses contain nothing but hygrometrical water. The observation is probably correct with respect to all but ammoniacal gas. From muriatic acid gas, confined in a thin glass globe, I have not been able to obtain any precipitation of water by applying a mixture of snow and muriate of lime, which produced a temperature of—20° Fahrenheit, and not of + 26°, as mis-printed in the text. [p. 360, l. 25.]

Ammoniacal

(B.) In the volume of the Arcueil Memoirs, which has gas did not oxidate iron wire, or form water, in the experiments of Berthollet, junr.

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* These arrived too late for insertion last month. For the paper see Vol. XXIV, p. 358.

just been quoted, Mr. Berthollet Junr. has published a Memoir on the Analysis of Ammonia. After failing in his attempts to oxidate iron wire, heated to redness, by a current of ammoniacal gas, he examined the fail of the generation of water, in a way somewhat different from that which I adopted. About twenty litres (upwards of 1200 cubic inches) of ammonia were decomposed by ignition, and the remaining gas passed through a tube cooled down to 0°; but no more water was deposited, than by an equal volume of ammoniacal gas which had not been ignited.

His analysis of ammonia by electricity and ignition,

On decomposing ammonia by electricity the resembling gasses bore to it the proportion of 2·04643 to 1, or were a little more than double. It does not appear, however, that Mr. Berthollet employed the precautions suggested by Mr. Davy, and essential to accurate results. One hundred measures of the permanent gas contained 75¼ hidrogen, and 24¾ nitrogen; and the same proportions were found in the products of ammonia decomposed by ignition. The proportion of hidrogen a little exceeds them, in these experiments, what is considered by Mr. Davy and myself as a correct determination, viz. 74 to 26.

and by deflagration with oxigen.

Mr. Berthollet, also, attempted the analysis of ammonia, by deflagrating it with oxigen; but as he employed an excess of the latter gas, he did not obtain accurate results; and abandoned the process without attempting to give it the precision, of which it is susceptible, when a deficiency of oxigen is used for the combustion nitric acid was formed in his experiments, and the detonating tubes were frequently broken, both of which inconveniences are completely avoided by using the proportions recommended in the text.

Biot and Arrajo's experiments.

According to the experiments of Biot and Arajo, the following numbers represent the specific gravity of ammonia, and of the gasses resulting from its decomposition, atmospheric air being taken as unity.

Specific gravity of am. gas and its parts.

Ammoniacal gas ·········	0·59669
Oxigen gas ············	0·07321
Nitrogen gas ···········	0·96913

Calculating on these data, and on the quantity of gasses, obtained by the decomposition of ammonia and their proportion to each other, as determined by his own experiments, Mr. Berthollet finds, that 100 parts by weight of ammonia consist of

Composition of ammoniacal gas.

81·13 nitrogen,
18·87 hidrogen.
———
100.

Precaution in the use of Volta's eudiometer.

The memoir of Mr. Berthollet concludes with some useful practical directions for obtaining accurate results by Volta's Eudiometer. The circumstance, which he finds to be of most importance, is such an adjustment of the gasses to each other, as will afford a pretty large residuum after combustion. If the residuary gas be only one sixth of the original mixture, air is extricated from the water, over which the detonation is made, in such a quantity as to prove a source of fallacy. When the residuum is one fourth, the effect may be overlooked; and, when it amounts to half the volume of the original gasses, the uniformity of the results show, that they are no longer influenced by the extrication of air from the water.

When oxigen and hidrogen are fired over mercury, if the oxigen be in excess, nitric acid & oxide of mercury are formed.

Comparing experiments on the detonation of hidrogen and oxigen gasses over mercury, with similar ones over water, Berthollet found, that they agree accurately when the hidrogen is in excess. But when the oxigen prevails, the diminution is greater than it ought to be. An experiment, made expressly to elucidate this point, showed, that, under these circumstances, the oxigen combines not only with the nitrogen which is unavoidably mingled with it, but also with the mercury. Two litres (upwards of 120 cubic inches) of a mixture of hidrogen and oxigen gasses, with an excess of the latter, were fired by forty successive explosions. Water was formed; and, beside this, an oxide of mercury in its lowest stage of oxidation. The liquid was strongly acid; potash threw down a black oxide; and paper afterward impregnated with the solution burned as if dipped into a solution of nitre.

Detonations over water preferred.

On the whole, Mr. Berthollet is disposed to expect greater precision from detonations made over water, than from those over mercury, especially when care is taken to have a residuum equal to half the joint bulk of the gasses before firing.

Errour corrected.

(C.) By an errour either of the manuscript or of the printer, carbonic acid gas is stated at the close of the paper [p. 373, l. 14], to have afforded, when electrized, a residuum equal to about $\frac{1}{40}$th of its bulk; whereas the quantity of permanent gas was *one half* after removing the undecomposed acid.

W. H.

Manchester, Dec. 16, 1809.

XII.

Observations on Acetic Acid: by Mr. J. B. MOLLERAT[*].

Strength of acetic acid not in proportion to its spec. grav.

THE examination of some specimens of acetic acid has shown me, that the strength of this fluid is by no means in the ratio of its density.

Two acids of very different strength were of the same gravity.

I examined an acid, which marked 9° on the areometer: its specific gravity, at the temperature of 12·5° R. [60·1° F.] is 1·063. This was compossd of 0·87125 acid and 0·12875 water; and 100 parts saturated 250 of crystallized subcarbonate of soda.

Another specimen of the same specific gravity, and marking the same degree on the areometer, at the same temperature, was composed of 0·41275 acid and 0·58725 water; and 100 parts saturated but 118 of crystallized subcarbonate of soda.

Number 1 crystallized completely between 10° and 11° R. [54·5° and 56·7 F.], and with difficulty became fluid at 18° [72·5°].

Number 2 did not crystallize at several degrees below 0.

The acid No. 1 is the strongest I could obtain, and I believe the purest that can exist. It is without empyreuma, &c. In this state the acid distils with very little fire, and extreme rapidity, without boiling.

No. 2 was composed artificially by adding to No. 1 the proportion of distilled water indicated by calculation, which determined exactly the quantity requiſite to produce 9° of density, and 118 strength of acid, denoted by the quantity of subcarbonate of soda it would saturate.

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[*] Annales de Chimie, vol. LXVIII, p. 88.

A weaker acid much heavier.

Having observed, that acetic acid saturating 250 of carbonate of soda, and crystallizing between 10° and 11° R. [54·5° and 56·7°], marked but 9° on the areometer; while another specimen of the acid, that marked 11·1° was not crystallizable even at 4° R. [41° F.], and saturated but 186·25 of subcarbonate of soda; I concluded,

General inferences.

1, That the areometer cannot measure the strength of acetic acid, without some preliminary steps:

2, That there must be a point discoverable by the areometer, at which the effective acid is in such proportion to the water, as to be capable of exerting in a sensible manner one of its physical properties, that was before imperceptible:

3, That this property must be a dilatability greater than that of water:

4, That this dilatability, apparent when the acid was in a certain proportion to the water with which I was yet unacquainted, must increase regularly in proportion as the quantity of water in the mixture diminished:

5, That the areometer would then regularly indicate the effective quantity of acetic acid in a mixture, by adopting an inverse ratio.

Experiments.

The following experiments confirmed my reasoning.

Addition of water increased the spec. grav. to a certain point,

I took 110 gr. of the acid No. 1, containing 0·87125 acid and 0·12875 water, and marking 9° by the areometer, and gradually added 35·941 gr. of distilled water to the 14·16 already in the 110 gr. of acid No. 1, in order that the mixture should contain the quantity of 50·101 gr. necessary to produce an acid of the composition of 65·65 acid to 34·35 water, being that of an acid marking 11·1° on the areometer.

When I had added 32·5 gr. of water to the 110 gr. of No. 1, the mixture indicated 11·3° on the areometer, which was the highest point of density of this acetic acid at the temperature of 12·5° R. [60·1 F.] Its composition then was 0·6725614 acid, 0·3274386 water.

and then diminished it.

On continuing to add water I brought the mixture down to 9° of the areometer, which was the density of the original acid of the experiment, before I had added 112·2 gr. of water to the 14·16 gr. it contained at first.

The following table represents the experiment made on 110 gr. of acetic acid, No. 1, indicating 9° on the areometer at the temperature of 12·5° R. [60·1° F.], its specific gravity being 1·063, and 100 parts saturating 250 of well crystallized subcarbonate of soda.

Tabulated results.

No.	Water added.	Areometer.	Specific grav.
1	10 gr.	10·6°	1·0742
2	12·25	11	1·0770
3	10	11·3	1·0791
4	10·5	10·9	1·0763
5	12	10·6	1·0742
6	11·5	10·4	1·0728
7	31*	9·4	1·0658
8	11	9·1	1·0637
9	37*	9	1·0630
	112·2		

Temperature raised by adding water.

Each addition of water to the mixture produce an increase of temperature, which I allowed to go off, noting the terms of the experiment only at the temperature of 12·5° [60·1°].

A distinct experiment on 100 gr. of the acid No. 1, to which I added 102 gr. of distilled water, ſhowed a rise of temperature equal to 3° R. [6.75° F.]. I added at first 29·54 gr. of water, which rose the temperature from 12·5° to 13·5°. I then added 72·46 gr. of water, which raised it from 13·5° to 15·5°.

General conclusions.

From what has been said we may conclude,

1st, That the rise of the areometer indicates the strength of the acetic acid, till the mixture is composed of 67·25614 acid and 32·74386 water. This term, at the temperature of 12·5° R. [60·1° F.], is expressed by 11·3 on the areometer, and its specific gravity is 1·0791.

2dly, That the strength of the acetic acid from 11·3° is indicated by the regular descent of the areometer on the mixture†.

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* I have copied the table exactly from the original, but the total would make 145·25, consequently there must be an errour of 33 in excess somewhere, and probably it is in the two numbers 31 and 37. C.

† I have repeated some of the experiments given in this paper, and my results differed but little from those here described. *Berthollet.*

XIII.

Extract of a Letter from Mr. W. MACLURE, *Member of the Philosophical Society of Philadelphia, to* J: C. DELAMETHERIE, *on the Volcanoes of Allot in Catalonia*[*].

Volcanic country in Catalonia.

I Shall communicate to you some mineralogical observations made by Mr. Fondi and me.

After having crossed the Pyranees on our way to Barcelona, we found lava and scoriæ in the bed of the Fluvia. We ascended toward the source of the river; we crossed ten miles of a volcanic country round Allot, and there observed several streams of lava, volcanic ashes, or puzzolana, crators not yet obliterated, &c. We had observed that this volcanic country extended fifteen or twenty miles to the south, beyond Amira, where in 1428 there was an eruption, that destroyed Allot, leaving but a single house standing. We found a great deal of lava in the bed of the river Ter; and near Massanit crossed an ancient stream of lava between two and three miles broad, in a state of decomposition, and covered with alluvial soil. From Massanit to Allot is near forty miles, so that the theatre of the volcanic action in this country is much more extensive than that of Vesuvius.

Extensive.

Granitic soil intersected by limestone.

Proceeding along the seashore from this place to Barcelona we crossed a granite country near fifty miles long. In two places it is intersected by a limestone soil.

Montferrat similar to other heights in the vicinity.

On the road to Cardona, by Montferrat, we found alternate stratifications of sandstone and puddingstone with clay, marle, and limestone, interposed occasionally for near fifty miles, similar to the stratification of Rigi, in the canton of Schwitz, which is higher than Montferrat: so that this celebrated mountain, which is said to be insolated, is but a portion of a bed of puddingstone and sandstone, which extends through a square of about fifty miles, and forms all the neighbouring mountains. These puddingstones are composed of four fifths limestone and pebbles: the cement is also calcareous, with some sandstones, a little quartz, and lapis Lydius. The latter has been improperly considered as a lava.

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[*] Journal de Physique, vol. LXVI, p. 220.

XIV.

XIV.

An Iris seen in the Dew, and a Lunar Iris. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Apparently rectilinear iris in the dew.

ONE sunny morning lately, as I was riding across a meadow, the sun being on my left, I perceived, to my right hand, upon the grass, a beautiful straight line of prismatic colours, formed by the refraction and reflection of the solar rays by the dew drops. The line extended from me all across the meadow, and formed with the shadow of a vertical object an angle of about 45°.

What shall we call this phenomenon? not a rainbow; for it is neither a bow, nor produced by rain: the French name, *arc-en-ciel*, is equally inapplicable; and so is the Italian, *l'arco baleno, l'arco celeste.*

Lunar iris.

A few weeks since, about nine o'clock in the evening, the weather being very frosty, I saw a beautiful circle, or iris, round the moon, exhibiting all the prismatic colours distinctly, whenever a thin fleecy cloud intervened. Perhaps these phenomena may be sufficiently rare, to merit being recorded in your celebrated Journal.

I am, Sir,

Dec. 14, 1809. Your obliged reader, A. M.

ANNOTATION.

The rainbow on the ground was observed at the beginning of the last century, by Mr. Langwith, Phil. Trans. No. 269; and B. Martin, in his Grammar, p. 213, correctly observes, that its figure will be an hyperbola, parabola, or ellipsis, according to the Sun's height. A. M. must have seen one of the legs of a curve of one of these descriptions. N.

St. Thomas's and Guy's Hospitals.

The Spring Course of Lectures at these adjoining Hospitals will commence the beginning of February.—For the particulars see our Journal, vol. XIV, p. 7.

To Correspondents.

As I cannot give a satisfactory answer to A. B., without a figure or two, I am under the necessity of deferring it till next month.—The papers of Mr. Barlow, Mr. Brewerton, Mr. Sheldrake, Mr. Ellis, and G. O., shall be given in our next.

METEOROLOGICAL JOURNAL,

For JANUARY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

DEC. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Day.	Night.
28	37°	38°	45°5	35°	30·07	Rain	Cloudy
29	44	46	48	39	29·75	Ditto	Ditto
30	42·5	47·5	48·5	43·5	29·74	Fair	Fair
31	48·5	47	50	44	29·94	Cloudy	Cloudy
JAN.							
1	47·5	48·5	50	43	30·10	Cloudy	Cloudy
2	46	45·5	48	42	30·22	Rain	Ditto
3	47	49	49·5	44	30·16	Cloudy	Ditto
4	45·5	48	49	44·5	30·28	Rain	Rain
5	45	43	46·5	40	30·34	Fair	Cloudy
6	43	45·5	47·5	41·5	30·30	Cloudy	Ditto
7	43·5	41	44	37·5	30·24	Ditto	Ditto
8	39	37	40	41	30·03	Rain	Ditto
9	45	45	47·5	40	29·85	Ditto	Ditto
10	41·5	43·5	45	41	30·00	Cloudy	Ditto
11	41·5	45	45·5	42	29·92	Ditto	Ditto
12	44	39	45	32	29·87	Rain	Ditto
13	34	32·5	34·5	26·5	29·94	Fair	Fair
14	27·5	26	30	26	30·02	Ditto	Ditto*
15	27	24	29	20	29·85	Ditto	Ditto
16	22·5	21	27	14	29·75	Snow	Hazy †
17	20·5	27	30	25·5	29·98	Fair	Ditto
18	31·5	28	35·5	25	30·21	Snow	Fog
19	27	25	32	21	30·24	Ditto	Hazy
20	21	27	29	27	30·18	Cloudy	Ditto
21	28	32	34·5	29	30·05	Ditto	Cloudy
22	31	32	34·5	29	29·95	Ditto	Rain
23	34·5	35	37	33	30·12	Ditto	Ditto
24	35	36	37·5	32.5	30·21	Ditto	Ditto
25	34	34·5	37	31	30·28	Ditto	Cloudy
26	32·5	35	36·5	31	30·33	Ditto	Ditto
27	32	33	34·5	30	30·25	Ditto	Ditto

* Snow in the night.

† Moon.

great extent of lever, which is constantly operating against the first mover, in consequence of the distance of the centre of support in large surfaces, if applied in the manner of wings.

Third: to combine strength and lightness with extent of surface.

The first obstacle less felt on a small scale.

A third and general obstacle is the mechanical skill required to unite great extension of surface with strength and lightness of structure; at the same time having a firm and steady movement in its working parts, without exposing unnecessary obstacles to the resistance of the air. The first of these obstacles, that have been enumerated, operates much more powerfully against aerial navigation upon a large scale, than against birds; because the small extent of their wings obliges them to employ a very rapid succession of strokes, in order to acquire that velocity which will give support; and during the small interval of the return of the wing, their weight is still rising, as in a leap, by the impulse of one stroke, till it is again aided by another. The large surfaces that aerial navigation will probably require, though necessarily moved with the same velocity, will have a proportionably longer duration both of the beat and return of the wing; and hence a greater descent will take place during the latter action, than can be overcome by the former.

Modes of obviating it on a large scale.

There appears to be several ways of obviating this difficulty. There may be two surfaces, each capable of sustaining the weight, and placed one above the other, having such a construction as to work up and down in opposition when they are moved, so that one is always ready to descend, the moment the other ceases. These surfaces may be so made, by a valvelike structure, as to give no opposition in rising up, and only to resist in descent.

The action may be considered either oblique, as in rotative flyers; alternately so, without any up and down waft, as in the engine I have ascribed to Mr. Degen; by means of a number of small wings in lieu of large ones, upon the principle of the flight of birds, with small intervals of time between each waft; and lastly by making use of light wheels to preserve the propelling power both of the beat and the return of the wings, till it accumulates sufficiently to elevate the machine, upon the principle of those birds which

Velocity of the oblique stroke.

Its advantage 10 to 1.

Hence its whole velocity must be 42·5 feet per second. Should the same surface be wafted downward, like a wing, with the hinder edge inclined upward in an angle of about 50°·40′ to the current, it will overtake it at a velocity of 3·5 feet per second; and as a slight unknown angle of resistance generates a pound pressure per square foot at this velocity, probably a waft of little more than 4 feet per second would produce this effect; one tenth part of which would be the propelling power. The advantage in favour of this mode of application, compared with the former, is rather more than ten to one.

Difficulties in mechanism to be overcome,

but enough has been done to promise ultimate success.

In combining the general principles of aerial navigation for the practice of the art many mechanical difficulties present themselves, which require a considerable course of skilfully applied experiments, before they can be overcome. But to a certain extent the air has already been made navigable; and no one, who has seen the steadiness with which weights to the amount of ten stone (including four stone, the weight of the machine) hover in the air, can doubt of the ultimate accomplishment of this object.

First difficulty: want of initial velocity.

The first impediment I shall take notice of is the great proportion of power, that must be exerted previous to the machine's acquiring that velocity, which gives support upon the principle of the inclined plane; together with the total want of all support during the return of any surface used like a wing. Many birds, and particularly water fowl, run and flap their wings for several yards before they can gain support from the air. The swift *(hirundo apus Lin.)* is not able to elevate itself from level ground. The inconvenience under consideration arises from very different causes in these two instances.—The supporting surface of most swimming birds does not exceed the ratio of $\frac{4}{10}$ths of a square foot to every pound of their weight: the swift, though it scarcely weighs an ounce, measures eighteen inches in extent of wing. The want of surface in the one case, and the inconvenient length of wing in the other, oblige these birds to aid the *commencement* of their flight by other expedients; yet they can both fly with great power, when they have acquired their full velocity.

Second: great extent of lever.

A second difficulty in aerial navigation arises from the great extent of lever, which is constantly operating against the first mover, in consequence of the distance of the centre of support in large surfaces, if applied in the manner of wings.

Third: to combine strength and lightness with extent of surface.

The first obstacle less felt on a small scale.

A third and general obstacle is the mechanical skill required to unite great extension of surface with strength and lightness of structure; at the same time having a firm and steady movement in its working parts, without exposing unnecessary obstacles to the resistance of the air. The first of these obstacles, that have been enumerated, operates much more powerfully against aerial navigation upon a large scale, than against birds; because the small extent of their wings obliges them to employ a very rapid succession of strokes, in order to acquire that velocity which will give support; and during the small interval of the return of the wing, their weight is still rising, as in a leap, by the impulse of one stroke, till it is again aided by another. The large surfaces that aerial navigation will probably require, though necessarily moved with the same velocity, will have a proportionably longer duration both of the beat and return of the wing; and hence a greater descent will take place during the latter action, than can be overcome by the former.

Modes of obviating it on a large scale.

There appears to be several ways of obviating this difficulty. There may be two surfaces, each capable of sustaining the weight, and placed one above the other, having such a construction as to work up and down in opposition when they are moved, so that one is always ready to descend, the moment the other ceases. These surfaces may be so made, by a valvelike structure, as to give no opposition in rising up, and only to resist in descent.

The action may be considered either oblique, as in rotative flyers; alternately so, without any up and down waft, as in the engine I have ascribed to Mr. Degen; by means of a number of small wings in lieu of large ones, upon the principle of the flight of birds, with small intervals of time between each waft; and lastly by making use of light wheels to preserve the propelling power both of the beat and the return of the wings, till it accumulates sufficiently to elevate the machine, upon the principle of those birds which run themselves up. This action might be aided by making choice of a descending ground like the swift.

A man can use great exertion for a short time,

With regard to another part of the first obstacle I have mentioned, viz. the absolute quantity of power demanded being so much greater at first than when the full velocity has been acquired; it may be observed, that, in the case of human muscular strength being made use of, a man can exert, for a few seconds, a surprising degree of force. He can run up stairs, for instance, with a velocity of from 6 to 8 feet perpendicular height per second, without any dangerous effort; here the muscles of his legs only are in action; but, for the sake of making a moderate statement, suppose that with the activity of his arms and body, in addition to that of his legs, he is equal to raising his weight 8 feet per second; if in this case he weighs 11 stone, or 154 pounds, he will be exerting, for the time, an energy equal to more than the ordinary force of two of Messrs. Boulton and Watt's steam horses; and certainly more than twelve men can bestow upon their constant labour.

Other expedients.

If expensive first movers be made use of, they may be so constructed, as to be capable of doing more than their constant work; or their power may be made to accumulate for a few moments by the formation of a vacuum, or the condensation of air, so that these expedients may restore at one time, in addition to the working of the engine, that which they had previously absorbed from it.

Method of obviating the second difficulty,

With regard to the second obstacle in the way of aerial navigation, viz. the length of leverage to which large wing-like surfaces are exposed, it may be observed, that, being a constant and invariable quality, arising from the degree of support such surfaces give, estimated at their centres of resistance, it may be balanced by any elastic agent, that is so placed as to oppose it. Let A and B, Pl. IV, fig. 1, be two wings of an aerial vehicle in the act of skimming; then half the weight of the vessel is supported from the centre of resistance of each wing; as represented by the arrows under them. If the shorter ends of these levers be connected by cords to the string of a bow C, of sufficient power to balance the weight of the machine at the points A and B, then the

by a counterbalancing spring,

the moving power will be left at full liberty to produce the waft necessary to bend up the hinder edge of the wing, and gain the propelling power. A bow is not in fact an equable spring, but may be made so by using a spiral fusee. I have made use of it in this place merely as the most simple mode of stating the principle I wished to exhibit. Should a counterbalancing spring of this kind be adopted in the practice of aerial navigation, a small well polished cylinder, furnished with what may be termed a bag piston (upon the principle made use of by nature in preventing the return of the blood to the heart, when it has been driven into the aorta, by the intervention of the semilunar valves) would, by a vacuum being excited each stroke of the wing, produce the desired effect, with scarcely any loss by friction*. These elastic agents may likewise be useful in gradually stopping the momentum of large surfaces when used in any alternate motion, and in thus restoring it during their return.

Or cylinder with a bag piston.

Farther uses of these.

Another principle, that may be applied to obviate this leverage of a wing, is that of using such a construction as will make the supporting power of the air counterbalance itself. It has been before observed, that only about one third of the wing in birds is applied in producing the propelling power; the remainder, not having velocity sufficient for this purpose, is employed in giving support, both in the beat and return of the wing.

Another method: making the air counterbalance itself.

Let A and B, fig. 2, be two wings continued beyond the pole or hinge upon which they turn at C. If the extreme parts at A and B be long and narrow, they may be balanced, when in the act of skimming, by a broad extension of less length on their opposite sides; this broad extension, like the

lower

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[*] I have made use of several of these pistons, and have no scruple in asserting, that for all blowing engines, where friction is an evil, and being very nearly airtight is sufficient, there is no other piston at all comparable with them. The most irregular cylinder, with a piston of this kind, will act with surprising effect. To give an instance; a cylinder of sheet tin, 8 inches long and 3¼ in diameter, required 4 pounds to force the piston down in 15 minutes; and in other trials became perfectly tight in some positions, and would proceed no farther. The friction, when the cylinder was open at both ends, did not exceed ½ an ounce.

Excellence of the bag piston.

lower part of the wing, will always give nearly the same support, and the propelling part of the surface will be at liberty to act unincumbered by the leverage of its supporting power. This plan may be modified many different ways; but my intention, as in the former case, is still the principle in its simplest form.

Third method: by preserving the parallelism of the wings.

A third principle upon which the leverage of a surface may be prevented is by giving it a motion parallel to itself, either directly up and down, or obliquely so. The surface A I, fig. 3, may be moved perpendicularly, by the shaft which supports it, down to the position K C: or, if it be supported upon two shafts with hinges at D and E, it may be moved obliquely parallel to itself into the position B L.

Fourth method: placing the hinge much below the plane of the wing.

The heron.

A fourth principle upon which the leverage may be greatly avoided, where only one hinge is used, is by placing it considerably below the plane of the wing, as at the point D, fig. 3, in respect to the surface A. It may be observed in the heron, which is a weak bird with an extended surface, that its wings curve downward considerably from the hinge to the tip; hence the extreme portion, which receives the chief part of the stroke, is applied obliquely to the current it creates; and thus evades in a similar degree the leverage of that portion of the supporting power, which is connected with the propelling power. These birds seldom carry their waft much below the level of the hinge of the wing, where this principle, so far as respects the supporting power, would vanish.

The third and fourth methods combined.

By making use of two shafts of unequal length, the two last mentioned principles may be blended to any required extent. Suppose one hinge to be at F, and the other at G, fig. 3, then the surface, at the extent of its beat, would be in the position of the line H M. If the surface A I, fig. 3, be supported only upon one shaft, N E, be capable of being forced in some degree from its rectangular position in respect to the shaft, and be concave instead of flat as here represented; then the waft may be used alternately backward and forward, according to the principles of the machine I have ascribed to Mr. Degen. This construction combines the principles of counterpoising the supporting power of one part of the surface, by that of an opposite part, when the machine is in the act of skimming; and likewise the advantages of the low hinge, with the principle of leaving little or no interval without support.

Mr. Degen's apparatus.

All that has hitherto appeared respecting Mr. Degen's apparatus is, that it consisted of two surfaces, which were worked by a person sitting between them. This statement communicates no real information upon the subject; for scarcely any one would attempt to fly without *two* wings; without these being equally poised by placing the weight *between* them; and also, without these surfaces being capable of receiving motion from his muscular action. I may be altogether mistaken in my conjecture; my only reason for ascribing this structure of mine to Mr. Degen's machine is, that, if it were properly executed upon this principle, it would be attended with success. The drawing, or rather diagram, which is given of this machine in the first part of my essay, is only for the purpose of exhibiting the principle in a form capable of being understood. The necessary bracings, &c., required in the actual execution of such a plan, would have obscured the simple nature of its action; and were therefore omitted. The plan of its movement is also simply to exhibit, in a tangible form, the possibility of effecting the intended alternate motion of the parachutes. The seat is fronted lengthwise for the purpose of accommodating the mode of communicating the movement.

First method.

A fifth mode of avoiding leverage is by using the continued action of oblique horizontal flyers, or an alternate action of the same kind, with surfaces so constructed as to accommodate their position to such alternate motion; the hinge or joint being in these cases vertical. In the construction of large vessels for aerial navigation, a considerable portion of fixed sail will probably be used; and no more surface will be allotted, towards gaining the propelling power, than what is barely necessary, with the extreme temporary exertion of the first mover, to elevate the machine and commence the flight. In this case the leverage of the fixed surface is done away.

General difficulties.

The general difficulties of structure in aerial vehicles, (arising from the extension, lightness, and strength required in them; together with great firmness in the working parts, and at the same time such an arrangement as exposes no unnecessary obstacles to the current,) I cannot better explain than by describing a wing, which has been constructed with a view to overcome them.

Practical attempt to overcome them.

Fig. 4 represents the shape of the cloth, with a perspective view of the poles upon which it is stretched with perfect tightness. Upon the point where the rods A and B intersect is erected an oval shaft; embracing the two cross poles by a slender iron fork; for the purpose of preserving their strength uninjured by boring. To this shaft are braced the ends of the pole B, so as to give this pole any required degree of curvature. The pole A is strung like a common bow to the same curve as the pole B; and is only connected with the upright shaft by what may be called a check brace; which will allow the hinder end of this pole to heel back to a certain extent, but not the fore end. The short brace producing this effect is shown in fig. 4. Fig. 5 exhibits the fellow wing to that represented in fig. 4, erected upon a beam, to which it is so braced, as to convert the whole length of it into a hinge. The four braces coming from the ends of this beam are shown: two of them terminate near the top of the centre of the other shaft; the others are inserted into the point C, fig. 4, of the bending rod. A slight bow, not more than three-eighths of an inch thick, properly curved by its string, and inserted between the hinder end of the pole A, and the curved pole C, completes the wing.

Weight, dimensions, and power of the wing.

This fabrick contained 54 square feet, and weighed only eleven pounds. Although both these wings together did not compose more than half the surface necessary for the support of a man in the air, yet during their waft they lifted the weight of nine stone. The hinder edge, as is evident from the construction, being capable of giving way to the resistance of the air, any degree of obliquity, for the purpose of a propelling power, may be used.

Principles of the construction.

I am the more particular in describing this wing, because it exemplifies almost all the principles that can be resorted to in the construction of surfaces for aerial navigation.

Diagonal bracing.

Diagonal bracing is the great principle for producing strength without accumulating weight; and, if performed by thin wires, looped at their ends, so as to receive several laps of cordage, produces but a trifling resistance in the air, and keeps tight in all weathers. When bracings are well applied, they make the poles, to which they are attached, bear endwise. The hollow form of the quill in birds is a very admirable structure for lightness combined with strength, where external bracings cannot be had; a tube being the best application of matter to resist as a lever; but the principle of bracing is so effectual, that, if properly applied, it will abundantly make up for the clumsiness of human invention in other respects; and should we combine both these principles, and give diagonal bracing to the tubular bamboo cane, surfaces might be constructed with a greater degree of strength and lightness, than any made use of in the wings of birds.

The surface of a heron's wing is in the ratio of 7 square feet to a pound. Hence, according to this proportion, a wing of 54 square feet would weigh about 7¾ pounds: on the contrary the wings of water fowl are so much heavier, that a surface of 54 square feet, according to their structure, will weigh 18½ lb. I have in these instances quoted nearly the extreme cases amongst British birds; the wing I have described may therefore be considered as nearly of the same weight in proportion to its bulk as that of most birds.

Proportions of weight to surface in the wings of birds.

Another principle exhibited in this wing is that of the poles being couched within the cloth, so as to avoid resistance. This is accomplished by the convexity of the frame, and the excessive lightness of the cloth. The poles are not allowed to form the edge of the wing, excepting at the extreme point of the bow, where it is very thin, and also oblique to the current. The thick part of this pole is purposely conveyed considerably within the edge. In birds, a membrane covered with feathers is stretched before the thick part of the bone of the wing, in a similar manner, and for the same purpose. The edge of the surface is thus reduced to the thickness of a small cord, that is sown to the cloth, and gives out loops whenever any fastening is required. The upright shaft is the only part that opposes much direct resistance to the current, and this is obviated in a great degree by a flat oval shape, having its longest axis parallel to the current.

Resistance avoided by placing the ribs remote from the edge.

The joint or hinge of this wing acts with great firmness,

Strength of the hinge.

in consequence of its being supported by bracings to the line of its axis, and at a considerable distance from each other; in fact the bracings form the hinge.

Means of communicating motion.

The means of communicating motion to any surfaces must vary so much, according to the general structure of the whole machine, that I shall only observe at present, that where human muscular action is employed, the movement should be similar to the mode of pulling oars; from which any other required motion may be derived; the foot board in front enables a man to exert his full force in this position. The wings I have described were wafted in this manner; and when they lifted with a power of 9 stone, not half of the blow, which a man's strength could have given, was exerted, in consequence of the velocity required being greater than convenient under the circumstances. Had these wings been intended for elevating the person who worked them, they should have contained from 100 to 150 square feet each; but they were constructed for the purpose of an experiment relative to the propelling power only.

Importance of avoiding resistance.

Avoiding direct resistance is the next general principle, that it is necessary to discuss. Let it be remembered as a maxim in the art of aerial navigation, that every pound of direct resistance, that is done away, will support 30 pounds of additional weight without any additional power. The figure of a man seems but ill calculated to pass with ease through the air, yet I hope to prove him to the full as well made in this respect as the crow, which has hitherto been our standard of comparison, paradoxical as it may appear.

Man equal to the crow in this respect.

The principle, that surfaces of similar bodies increase only as the squares of their homologous lines, while their weights, or rather solid contents, increase as the cubes of those lines, furnishes the solution. This principle is unanimously in favour of large bodies. The largest circle that can be described in a crow's breast is about 12 square inches in area. If a man exposes a direct bulk of 6 square feet, the ratio of their surfaces will be as 1 to 72; but the ratio of their weight is as 1 to 110; which is 1½ to 1 in favour of the man, provided he were within a case as well constructed for evading resistance, as the body of the crow; but even supposing him to be exposed in his natural cylindric shape, in the foreshortened posture of sitting to work his oars, he will probably receive less resistance than the crow.

Solid of least resistance requires to be determined.

Newton's theorem.

It is of great importance to this art, to ascertain the real solid of least resistance, when the length or breadth is limited. Sir Isaac Newton's beautiful theorem upon this subject is of no practical use, as it supposes each particle of the fluid, after having struck the solid, to have free egress; making the angles of incidence and reflection equal particles of light seem to possess this power, and the theory will be true in that case; but in air the action is more like an accumulation of particles, rushing up against each other, in consequence of those in contact with the body being retarded. The importance of this subject is not less than the difficulties it presents; it affects the present interests of society in its relation to the time occupied in the voyages of ships; it will have still more effect when aerial navigation, now in its cradle, is brought home to the uses of men. I shall state a few crude hints upon this point, to which my subject has so unavoidably led, and on which I am so much interested, and shall be glad if in so doing I may excite the attention of those, who are competent to an undertaking greatly beyond my grasp.

Mode of considering the problem.

Perhaps some approach toward ascertaining the actual solid of least resistance may be derived from treating the subject in a manner something similar to the following. Admit that such a solid is already attained (the length and width being necessarily taken at pleasure). Conceive the current intercepted or disturbed, by the largest circle that can be drawn within the given spindle, to be divided into concentric tubular laminæ of equal thickness. At whatever distance from this great circle the apex of the spindle commences, on all sides of this point the central lamina will be reflected in diverging pencils, (or rather an expanding ring,) making their angles of incidence and reflection equal. After this reflection they rush against the second lamina and displace it: this second lamina contains three times more fluid than the first; consequently each pencil in the first meets three pencils in the second; and their direction, after the union, will be one fourth of the angle, with respect to the axis, which the first reflection created. In this direction these

these two laminæ proceed till they are themselves reflected, when they (considered as one lamina of larger dimensions) rush against the third and fourth, which together contain three times the fluid in the two former laminæ, and thus reduce the direction of the combined mass to one fourth of the angle between the axis and the line of the second reflection. This process is constant, whatever be the angles formed between the surface of the actual solid of least resistance at these points of reflection, and the directions of the currents thus reflected.

Inference.

From this mode of reasoning, which must in some degree resemble what takes place, and which I only propose as a resemblance, it appears, that the fluid keeps creeping along the curved surface of such a solid, meeting it in very acute angles. Hence, as the experiments of the French Academy show, that the difference of resistance between the direct impulse, and that in an angle of six degrees, on the same surface, is only in the ratio of 10 to 4, it is probable, that in the slight difference of angles that occur in this instance, the resistances may be taken as equal upon every part, without any material deviation from truth. If this reasoning be correct, it will reduce the question, so far as utility is concerned, within a strictly abstract mathematical inquiry.

Figure of the hinder part important.

It has been found by experiment, that the shape of the hinder part of the spindle is of as much importance as that of the front, in diminishing resistance. This arises from the partial vacuity created behind the obstructing body. If there be no solid to fill up this space, a deficiency of hydrostatic pressure exists within it, and is transferred to the spindle. This is seen distinctly near the rudder of a ship in full sail, where the water is much below the level of the surrounding sea. The cause here, being more evident, and uniform in its nature, may probably be obviated with better success; in as much as this portion of the spindle may not differ essentially from the simple cone. I fear however, that the whole of this subject is of so dark a nature, as to be more usefully investigated by experiment, than by reasoning; and in the absence of any conclusive evidence from either, the only way that presents itself is to copy nature; accordingly I shall instance the spindles of the trout and woodcock, which, lest the engravings should, in addition to the others, occupy too much valuable space in your Journal, must be reserved to a future opportunity.

II.

On the Use of the Camera Lucida as a Substitute for the Camera Obscura. In a Letter from Mr. T. SHELDRAKE.

To Mr. NICHOLSON.

SIR,

I AM glad to find, that my communication has induced your correspondent, Mr. Bate, to illustrate the camera lucida; and, that it may receive all the light that can be thrown upon it, I beg leave to resume the subject.

The camera lucida not an advantageous substitute for the camera obscura.

I propose to show, that the camera lucida does not possess one property, that will induce an artist, who has sufficient motives to make use of the camera obscura, to lay aside that instrument, and substitute the camera lucida in its stead. In doing this I must be understood to confine my investigation to *this single* point, without attempting to affect the general character of the camera lucida, which *I know has some*, and *may have many more* valuable properties, with which I am not acquainted.

I beg leave to say in plain terms, that I question no other property of the camera lucida; and I beg farther, to prevent me from being misunderstood, to describe the purposes for which it is desirable to make use of either instrument.

Use of the camera obscura.

It is to be presumed, that whoever undertakes to draw from nature has acquired *some* power of representing the objects that appear before him. When he wishes to draw a view, he surveys the scene, and determines what objects are to be concluded in his design: he determines their relative sizes and positions, and executes his view well or otherwise, according to the power of practice that he has acquired. But all this requires mental exertion, as well as practical skill, and a portion of time proportioned to the care and skill that is to be employed. It is with a view to save time, and ensure a more perfect execution of the work, that the assistance of the camera obscura is desirable.

For example—A gentleman who draws well, but not expeditiously, gets into a picturesque country, and wishes to draw views of the scenery he sees; he tries, and does not succeed to his liking; he sets up his camera obscura, and succeeds much better. Or, an artist is employed to travel through a beautiful country, which he can never see again; he wishes to secure views of every fine scene that he sees, and make the best use of his time: in consequence he uses the camera obscura, and secures a much *greater number* of views, better executed, and done in much less time than he would have produced them by drawing in the usual way.

Compared with the camera lucida.

This is the test to which I wish to bring the camera lucida; which, I contend, is in no respect superior, and in many respects inferior, to the camera obscura. First, in respect to *portability*, which may at first sight be thought its strongest point.

At least equally portable.

If our amateur or artist has determined to make his drawings of a large size, suppose each drawing on half a sheet of imperial paper, which measures about 22 inches by 15, his camera obscura will be contained in a box about 24 inches square, and 6 or 8 deep; but this will likewise contain more paper than he can use in one day, and all his drawing materials. As the camera lucida is contained in a box about 9 inches by 3, and one inch thick; as this may be put into the pocket; they who should see both the instruments in the maker's shop, and be told that with the small one they can do every thing that can be done with the large one, would of course take it in preference; but mark the consequence.

Mr. Bate has told us, that "in copying a landscape the "instrument is to be fixed on a steady table or board, on "which a sheet of paper is to be stretched, and the prism "brought over the middle of it." Of course, the table, or board, on which the paper is to be stretched, must be equal in size to the box of the camera obscura, which the artist would use with the same paper, and therefore is an incumbrance of equal magnitude. Consequently, although it is undoubtedly true, that the camera lucida, considered by itself, is infinitely more portable than the camera obscura, this advantage would vanish the moment an artist or amateur should determine to make a practical use of it for the purpose that I have described; because he could not make use of it without carrying his table or stand with him, and thus instead of avoiding *all* incumbrance, only substitute one incumbrance for another.

Superior brilliancy of the camera lucida no advantage.

The superior brilliancy and distinctness, with which the camera lucida can represent objects under *some circumstances*, gives it no advantage whatever over the camera obscura as an instrument for drawing; because, as Mr. Bate tells us, and I have verified the fact, *we must destroy the superior brilliancy of those objects, before we can get a sight of the pencil, with which we are to represent them*—a conclusion which I think is to be fairly drawn from his own statement.

Obstacles to the use of the camera lucida,

Mr. Bate has said: "Though hitherto omitted, it is proper to notice the frequent impediments to an extent of "view, arising from the projection of near objects; part of "the head dress in particular are sometimes unsuspected "obstructions, and the brim of the hat the most formidable "of all."

This is expressed with delicacy and caution, but it will be necessary to state the fact in plainer terms: it is indispensably necessary, that the light should *fall upon the pencil and the paper*, or the view and the pencil will be seen so very imperfectly, that it will be almost impossible to make any kind of sketch, however imperfect, with it. This brings the two instruments before us in a way that will show the great superiority of the camera obscura. The time that artists choose to draw from nature in the country is the fine weather in summer, more frequently when the sun shines than at any other period; under these circumstances they will never attempt to draw with the sun shining in their faces; they will generally keep it upon their left hand, or perhaps turn their backs upon it, in order that the shadow of their persons may fall upon the paper, and prevent it from dazzling their eyes, which, in addition, are shaded by the brim of their hat. If an artist chooses to draw in the camera obscura, he procures all these advantages in a still greater

none to that of the camera obscura.

greater degree; his eyes will be more completely shaded from the sun, he may keep his hat on to shade his head, and, if he was so luxuriously disposed, might station a servant to hold a large parasol to shade him completely from the sun, without the least impediment to his work. But if he should choose to draw with the camera lucida, he must abandon all these advantages; turn his face towards the sun if it was shining, let its light fall full upon his paper, the reflection from which must reverberate into his eyes, and take off his hat, that the agreeable shade it *might* afford might not prevent him from seeing his pencil, or the view he intended to draw. The luxuries of such a situation may be easily conceived; and I believe that very few would wish to enjoy them, who was aware of them.

Farther comparison.

Having said so much on the preceding, which I am tempted to call auxiliary points of comparison, I shall proceed to compare the simple act of drawing with each of these instruments, beginning with the camera lucida.

Mode of drawing with the camera lucida,

I have repeated the methods of drawing with this instrument as described by Mr. Bate, and find them correct: it is to be understood from them, that, in order to draw any view, you must fix the eye to the eye hole, and move the head upwards, downwards, or sideways, to get a sight of the view, taking care to move the pencil in a direction opposite to that in which the head is moved, so as to pass it over the objects by degrees, as the motion of the head makes them appear upon the paper. That it is *possible* to do this, there is no doubt; but, when done, it will produce something very different from the *spirited or true sketch of an artist*, since the whole must be effected by the tremulous creeping movement of the hand, *feeling* its way in twilight, rather than firmly representing that which the artist sees.

and with the camera obscura.

The whole of this is directly the reverse of what may be effected in the camera obscura. Whether the object of an artist be to study the minutiæ of the scene before him with tasteless, insipid correctness, of which the works of Vanderheyden are the most eminent example; or whether he wishes to combine truth of representation, with tasteful selection of object, and strong delineation of character; the subject he chooses is placed completely in his power at *one* *view*, and he has nothing to do but exert the power he has to fix it upon his paper with more rapidity, than he possibly can in any other manner.

Having now, I believe, proved that which I undertook to prove, it is time to take leave of the subject. I am sensible, that the camera lucida has many valuable properties, which it is not my object to investigate. I was induced to search for it by Dr. Wollaston's declaration, that it *possessed many advantages over the camera obscura:* I tried it, and *found it wanting*. Doubtful whether the deficiencies I found were in the instrument itself, or in my manner of using it, I sought for information by stating in your Journal the inconveniences *that I found* in attempting to use it. Mr. Bate has shown, that, by trusting to the only public information that has been given, I fell into some errours, which he has corrected; and, I think, enabled me to show, that, whatever other valuable properties it may possess, it has none that will induce an artist, who has chosen to use the camera obscura in the practice of his profession, to lay that instrument aside, and adopt the camera lucida in its stead.

I am, Sir,

Yours, &c.

No. 50, Strand, T. SHELDRAKE.
Dec. 8, 1809.

III.

On the Acids produced by treating Ginger Root with Nitric Acid. By T. LE GAY BREWERTON, *Fellow of the Royal Physical Society, Edinburgh.*

IN No. 105 of this Journal, a new acid is announced, and proposed to be called the zingiberic. To obtain this new acid, and examine its combinations with salifiable bases, gave origin to the experiments below.

Zingiberic acid announce .

Attempt to obtain it.

Exp. 1. One ounce of Jamaica ginger root was digested five days in six ounces of nitric acid, S. Grav. 1·350, then diluted with water, kept boiling for twelve hours, saturated with carbonate of lead; the solution filtered, &c.; following, throughout the whole process, as nearly as possible the directions given by your correspondent, and ultimately a salt (*a*) was obtained "similar in appearance to short white pieces of raw silk," mixed with a little powdery matter of a yellowish white colour. Here I must observe, that the carbonate of lead which I employed was the *white lead* of the shops, and contained a considerable portion of carbonate of lime.

Salt obtained on the addition of dilute sulphuric acid.

A little of the liquor obtained after the second filtration was accidentally mixed with diluted sulphuric acid, when unexpectedly crystals (*b*) were deposited nearly similar to those obtained by evaporation, and nearly equal in quantity; although the liquor evaporated must, before evaporation, have exceeded that mixed with the diluted sulphuric acid, at least, four times.

Neither of these an acid.

The salt (*a*) and crystals (*b*), when separated from the supernatant liquor and washed, were not acid. Having failed in finding the acid in question without being able to attribute my failure to any particular cause, I determined to renew the search, altering the proportion of nitric acid employed.

Second attempt to obtain it.

Exp. 2. One ounce and a half of ginger being treated with six ounces of nitric acid in the same manner as in Exp. 1, of the liquor obtained after the second filtration four ounces were evaporated, and eight grains and a half of a salt (*c*) precisely similar in appearance to (*a*) were obtained. To another four ounces of the same liquor sulphuric acid was added, and after standing several hours a number of crystals (*d*) were deposited; which, after separating, washing, and drying at a temperature of about 200°, weighed six grains and a half, and were similar in appearance to the crystals (*b*) obtained in the first experiment. Neither the crystals (*d*) nor the salt (*c*) possessed any acidity; and from my only obtaining salts destitute of acidity, I attempted to obtain the same products without the employment of ginger.

Exp. 3.

Attempt to obtain similar salts without ginger.

Exp. 3. To nitric acid, as used in the above experiments, white lead was added until effervescence ceased to be produced; and to this liquor sulphuric acid was added until it ceased to throw down any precipitate; then the liquor was filtered, and sulphuric acid again added without regard to quantity. In half an hour crystals were observed depositing*, and when the deposition had ceased they were separated, and fresh additions of sulphuric acid made to the residuary liquor, so long as any crystals could be obtained. The crystallized matter (*e*) obtained in this experiment precisely resembled in appearance the crystals (*b*) in Exp. 1, and (*d*) in Exp. 2.

Properties of the crystallized matter obtained in the above experiments.

Properties of the crystallized matter.

The products (*a*), (*b*), (*c*), (*d*), and (*e*), were of a white colour, crystals filiform, or capillary; the crystals of (*b*), (*d*), and (*e*), were longer, more slender, and more flexible than those of (*c*) and (*a*) Taste of all the crystallized products insipid.

The crystallized matters (*a*), (*b*), and (*c*), were examined separately, and found to have the following chemical properties in common †. On exposure to a red heat they decrepitate slightly, become friable, more opake, diffusible in water, and when in a certain proportion dry quickly into a solid mass. They are sparingly soluble in water, the solution gives a dense white precipitate with barytic water, and also with oxalic acid a dense white precipitate insoluble in vinegar.

Sulphate of lime: but this could not have prevented the formation of zingiberic acid.

From these properties I infer, that the crystallized matter obtained in the above experiments is sulphate of lime, and must have originated from substituting white lead of the shops for carbonate of lead; however the mixture of carbonate of lime with carbonate of lead could not have prevented me from obtaining the acid of ginger, because zingiberic acid must have either a less, a greater, or an equal affinity with sulphuric acid for lime; if a less, the crystals of zingiberic acid ought to have been mixed with the sulphate; if a greater, the zingiberate of lime alone, or mixed with the sulphate, supposing more lime to be present than the zingiberic acid could combine with, should have been formed; and if an equal, a mixture of zingiberate and sulphate ought also to have been formed. However, to remove all doubts, experiment was again tried.

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* The fact of sulphuric acid causing the deposition of sulphate of lime from a state of solution to me is not a little surprising, however it may perhaps be well known to those more versed in chemical experiments, and in no manner puzzling. B.

† As the experiments which led to the ascertaining of these properties can be of no particular utility, I have omitted inserting them. B.

Farther attempt to obtain the acid.

Exp. 4. One ounce of Jamaica ginger coarsely powdered was infused in three ounces of nitric acid, S. G. about 1·350, at a temperature of between 45° and 55°. In a few hours the liquor assumed an olive green colour, and the ginger was reduced to a pulpy mass. During the infusion of the ginger nitrous gas was constantly disengaged, but not in any considerable quantity. At the expiration of four days six ounces of water were added, and the liquor boiled for about ten hours, water being added occasionally so as to keep the liquor about the same quantity. After boiling about five hours, the fumes disengaged ceased to become orange on mixing with the external air, but continued to smell acid, and redden litmus paper the whole time. The liquor was now of a light orange colour, much of the substance of the ginger was dissolved during the process, and the remainder was separated by filtration.

The filtered liquor neither gave cloud nor precipitate with lime or barytic water.

Crystals obtained,

(A) About two ounces of the filtered liquor were exposed to the heat of a lamp, when, after boiling briskly about five minutes, nitrous gas was disengaged, and in a little longer time the liquor became rather turbid; upon which the lamp was removed, and on cooling the liquor deposited about a drachm of crystals. These crystals were quadrilateral prisms with unequal sides, but the opposite sides equal; they were intensely acid, readily soluble in water, the solution affording with lime water a dense white precipitate insoluble in vinegar. Hence I presume these were crystals of oxalic acid.

apparently of oxalic acid.

(B) About two ounces of the filtered liquor were exposed to a temperature below ebullition (however from inattention the liquor boiled about two minutes) until evaporated to about one sixth. This evaporated liquor on cooling was of the consistence of a sirup, and amongst it a few small quadrilateral prismatic crystals could be observed; it smelled something like burnt sugar, tasted not very acid, was readily soluble in water, the solution affording with lime water a slowly deposited white flocculent precipitate, almost completely soluble in vinegar. Hence I infer this evaporated siruplike product to be principally malic acid; and the few crystals observable, oxalic acid.

Crystals of oxalic acid with malic acid.

From the results of No. 4 I think it but fair to conclude, that the acids produced by the mutual action of nitric acid and ginger root are the malic and the oxalic; the latter, as is the case with many other vegetable substances, the result of a more complete reaction of the elements of the two agents; and this conclusion may be more readily admitted, when it is considered, that Exp. 1, 2, 3, in no manner argue against it.

No peculiar acid in ginger.

IV.

On the Method of transforming a Number from one Scale of Notation to another, and its Application to the Rule of Duodecimals. By Mr. Peter Barlow.

To Mr. NICHOLSON.

SIR,

It is rather the novelty, than the utility, of the rules contained in the following paper, that has induced me to send it for insertion in the Philosophical Journal, though it may possess some little of the latter; and if it will not exclude matter of more interest, it may probably be acceptable to some of your mathematical readers: in which case it is very much at your service.

On transferring numbers from one scale to another.

Yours, &c.

Royal Military Academy, Woolwich, P. BARLOW.
Jan. 29th, 1810.

Comparison of different scales of notation.

Before we enter upon the subject of transformation, it may not be amiss to make a few observations on the nature of notation in general, and the comparative advantages and disadvantages of particular systems with regard to the decimal scale of notation, which is almost universally adopted by all nations that have any knowledge of arithmetical computations.

Notation by tens.

Its origin.

This singular coincidence in the division of numbers into periods of tens is a subject, that has been noticed by philosophers ever since the time of Aristotle, and is now generally attributed to the formation of man; that is to say, his having ten fingers, by the assistance of which, in all probability, calculation, or at least numbering, was first effected. See *Montucla's Histoire des Mathematiques*, vol. I.

Its use a late improvement.

Greek notation.

Our present scale of notation, however, though founded on this principle, was not the immediate consequence of this division, but was an improvement introduced a long time afterward; as is evident from the works of the Greeks, who, notwithstanding they divided numbers into periods of tens, had no idea of the present system of arithmetic, the great and important advantage of which is, the giving to every digit a local, as well as a simple or natural value, and by this means being able to express any number, however large, by ten numerical characters; and, for want of which, the Greeks employed *twenty-seven*, we may say *thirty-six*, different symbols, with which they could not for a long time express a number above 10000; but they afterwards extended it to the squares of this number. See an ingenious essay on this subject, by Dalembert, at the end of the French translation of the works of Archimedes.

Advantages of our method.

Not the best possible.

I have already observed, that the advantage of our present system of notation consists in giving to each character a local value, by which they increase in a tenfold proportion from the right hand towards the left; but in this I wish to be understood as merely speaking of the method, and not of the number that is selected for the radix of the system; for there is no doubt, that either 6 or 12 would have better answered this purpose, particularly the latter.

It would be extending this paper to too great a length to enter into the peculiar advantages of this or that system of notation; particularly, as the number 10, though not the best that might have been adopted, is very proper for the purpose; and as the advantages possessed by the other are not such as can lead us to expect, or even to wish, that it should ever be substituted for that, which long established custom has rendered so familiar to all our ideas of numbers. I shall therefore, in what follows, limit myself entirely to the method of transforming a number from one scale to another, and showing its application to the rule of duodecimals, which, so far as relates to the converting of a number from our own scale to any other, and its application to the rule above mentioned, I conceive to be new, and more simple than any other that I am at present acquainted with.

Method of transforming a number from one scale to another.

Let r be the radix of any system, and a, b, c, d, &c., the digits, by which any number (N) is expressed in that system, then we have

$$N = r^n a + r^{n-1} b + r^{n-2} c + r^{n-3} d, \text{ \&c.} + 9.$$

Thus 1746 in the decimal scale may be expressed by $10^3.1 + 10^2.7 + 10^1.4 + 6$, and, in the duodenary scale, 8467 may be represented by $12^3.8 + 12^2.4 + 12^1.6 + 7$. And so on for others; where it will be readily observed, that the number of characters, including the 0, in any scale of notation, will never exceed the number that expresses the radix of that system. Thus, in the binary scale only two characters are wanted, namely 1 and 0; in the senary, six; in the decimal, ten; and in the duodenary, twelve; two additional characters being necessary for expressing 10 and 11, those I shall represent thus $10 = \varphi$, and $11 = \gamma$, so that the digits of this system are as follows.

Number of characters must equal the root of the system.

$$0,\ 1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 7,\ 8,\ 9,\ \varphi,\ \gamma.$$

Duodenary characters.

Now, r being the radix of any system, and a, b, c, d, &c. the digits of a number in that system; also r' the radix, and a', b', c', d', &c. the digits in any other system: then, in case of equality, we shall have $r^n a + r^{n-1} b + r^{n-2} c + r^{n-3} d$, &c. $= r'^m a' + r'^{m-1} b' + r'^{m-2} c'$, &c.; and, therefore, in converting a number from one scale to another, this equality must be established, and we must solve the above equation, the value of all the numbers being given on one side, but on the second, only the value of r', the other letters representing unknown quantities: whence, as there are several unknown quantities, and only one equation, it is evident, that there are, in an algebraical point of view, various values of a', b', c', d', &c., as also of m, that will establish this equality; but the forms being limited to a certain number of digits, less than r', only one possible answer can be obtained, and this is best effected independently of any algebraical consideration.

Numbers easily transformed into the scale to which we are accustomed.

There is no difficulty in converting a number from any other scale of notation into our own, for we have only to express the numbers by means of the foregoing formula, and then collect the value of the separate terms.

Example 1.

Thus, to transform the number expressed by 74671, in the duodenary scale, into the decimal notation.

Here $74671 = 12^4.7 + 12^3.4 + 12^2.6 + 12.7 + 1$, also

$$\begin{array}{lrr} \text{And} & 12^4.7 = & 145152 \\ & 12^3.4 = & 6912 \\ & 12^2.6 = & 864 \\ & 12.7 = & 84 \\ & 1 & 1 \\ \hline \text{therefore } 74671 & = & 153013 \end{array}$$

Example 2.

Again, convert 3441 from the senary to the decimal scale.

First $3441 = 6^3.3 + 6^2.4 + 6.4 + 1$.

$$\begin{array}{lrr} \text{and} & 6^3.3 = & 648 \\ & 6^2.4 = & 144 \\ & 6.4 = & 24 \\ & 1 = & 1 \\ \hline \text{therefore } 3441 & = & 817 \end{array}$$

Example 3.

Convert 1010110 from the binary to the decimal scale of notation.

Here $1010110 = 2^6 + 0 + 2^4 + 0 + 2^2 + 2 + 0 = 86$, so that 1010110, in the binary scale, is expressed by 86 in the common notation.

We have, therefore, no difficulty in transforming a number from any other scale to our own. But it is not so easy to convert a number from the decimal to another notation, though the operation is of a similar nature; but the difficulty arises in our not being so ready in the use of the other scales.

The converse more difficult.

In this case the given number is always of the form $10^n a + 10^{n-1}b + 10^{n-2}c$, &c., and the readiest way to transform it is, first to convert each of the powers of 10, beginning with the lowest, into the required scale, then each of the given terms in the formula into the same, the sum of which will be the answer. When it is only necessary to observe, that in all the operations of multiplication, and addition, we must divide by the radix of the system, setting down the overplus, and carrying the quotient, instead of dividing by 10, as is done in common arithmetic.

Rule.

As an example, let it be required to transform 1728 to the duodenary scale.

Example 4.

First
$$\begin{aligned} 10 &= \varphi \\ 10^2 &= \varphi \times \varphi = 84 \\ 10^3 &= 84 \times \varphi = 6\gamma 4 \\ 10^4 &= 6\gamma 4 \times \varphi = 5954 \\ &\text{\&c.} \end{aligned}$$

Also $1728 = 10^3 + 10^2.7 + 10.2 + 8.$

$$\begin{aligned} 8 &= 8 = 8 \\ 10.2 &= \varphi \times 2 = 18 \\ 10^2.7 &= 84 \times 7 = 4\varphi 4 \\ 10^3.1 &= 6\gamma 4 \times 1 = 6\gamma 4 \\ \text{therefore } 1728 &= \overline{1000} \text{ Ans.} \end{aligned}$$

This example being understood, the transformation in other cases will become very easy: thus in the following examples, transform 71671 to the duodenary scale.

First, $71671 = 10^4.7 + 10^3.1 + 10^3.6 + 10.7 + 1.$

Example 5.

Now

$$\begin{array}{lrcrcr}
\text{Now} & 1 = & & 1 & = & 1 \\
& 10 \,.\, 7 = & & \varphi \times 7 & = & 5\varphi \\
& 10^2 . 6 = & & 84 \times 6 & = & 420 \\
& 10^3 . 1 = & & 6\gamma 4 \times 1 & = & 6\gamma 4 \\
& 10^4 . 7 = & & 5954 \times 7 & = & 34614 \\
\hline
& & & & & 35587 \; \textit{Ans.}
\end{array}$$

Example 6. Transform 11111 to the senary scale.

Here $11111 = 10^4 + 10^3 + 10^2 + 10 + 1$.

$$\begin{array}{rclcr}
1 & = & 1 & = & 1 \\
10 & = & 14 & = & 14 \\
10^2 & = & 14 \times 14 & = & 244 \\
10^3 & = & 244 \times 14 & = & 4344 \\
10^4 & = & 4344 \times 14 & = & 114144 \\
\hline
& & & & 123235 \; \textit{Ans.}
\end{array}$$

Example 7. Convert 1248 into the binary scale.

First $1248 = 10^3 . + 10^2 . 2 + 10 . 4 + 8$.

$$\begin{array}{lcrcr}
10 & = & 1010 & & \\
10^2 & = & 1010 \times 1010 & = & 1100100 \\
10^3 & = & 1100100 \times 1010 & = & 1111101000 \\
\text{again } 8 & = & 1000 & = & 1000 \\
10 \,.\, 4 & = & 1010 \times 100 & = & 101000 \\
10^2 \times 2 & = & 1100100 \times 10 & = & 11001000 \\
10^3 & = & & & 1111101000 \\
\hline
& & & \textit{Ans} \cdots\cdots\cdots & 10011100000
\end{array}$$

Easy in practice. Various other examples might be given, but the above are sufficient for pointing out the method of transformation, which will become remarkably easy with a very little practice. We might also pursue the subject still farther, by converting a number from one scale to another, neither of which is the decimal; but this is perhaps better effected, by first transforming the numbers into the decimal scale, and afterwards into that proposed.

Application I shall conclude now with showing the application of what has been said to the rule of duodecimals, or cross multiplication.

to the rule of duodecimals, or cross multiplication.

To multiply feet, inches, parts, &c. by similar denominations.

Rule. Transform the number of feet, if above 12, into the duodenary scale, and set the inches, parts, &c. as decimals; then multiply as in common arithmetic, except, carrying for every 12, instead of every 10, as in common operations.

Example. Multiply 3 feet, 5 inches, 7 parts, by 3 feet, 11 inches, 2 parts.

Here 3·57
3·γ2

6γ2
3215
γ49

12·8542 = 14 feet, 8′ 5″, 4‴, 2⁗.

Multiply 17 feet, 3′, 4″ by 19 feet, 5′, 11″.

15·34
17·5γ

13φ08
7248
φ0γ4
1534

240·9688 = 336 ft. 9′, 6″, 8‴, 8⁗.

Find the solidity of a cube, the side of which is 3 feet, 7 in. 7 parts.

3·77
3·77

2151
2151
φφ9

11·2361
377

784067
784067
336φ63

3γ·φφφφ17 = 47 ft. 10′, 10″, 10‴, 10⁗, 1, 7.

And

This preferable to the common rule.

And thus may any operation in duodecimals be performed as readily as common multiplication, and the result obtained to the greatest degree of accuracy, on both of which accounts, I consider is as preferable to the rule commonly given for this purpose.

V.

On the Propagation of Sound through unelastic Fluids. In a Letter from FRANCIS ELLIS, *Esq.*

To Mr. NICHOLSON.

SIR,

Propagation of sound through water.

I Have accidentally become acquainted with a fact relative to the propagation of sound through unelastic fluids, which seems new; and if really so, you may perhaps deem it of sufficient interest, to be made known through the medium of your Journal.

The sound occasioned by the condensation of steam.

Bathing in a warm bath at Cheltenham, and plunging my head under the water, I was suprised by a harsh, disagreeable sound, and jar through both ears, resembling the sound and sensation, that would, I imagine, be received, were a sheet of tin laid on each ear, and struck simultaneously with two small hammers. Induced by curiosity to examine whence this could proceed, I observed, that the bath, which is ten or twelve feet square, was heated by steam conveyed into it at one of its angles by a metallic tube; and that the condensation of the steam produced at intervals in this tube a dull clicking sound. By immersing one ear in the bath, and keeping the other in the air, I ascertained, that it was this sound altered and augmented by being propagated through the water of the bath, which had caused the harsh snapping noise, and jarring sensations, that I have already described. The distance of my head from the entrance of the steam-tube into the bath was seven or eight feet: and it is remarkable, that, when it was submerged, I had distinct, though simultaneous sensations of the jarring sound in each ear.

I am, Sir,

Your very obdient servant,

FRANCIS ELLIS.

Somerset Place, Bath,
10*th Dec.* 1809.

Observ-

OBSERVATION.

This noise is of the same nature as that of simmering; (See Philos. Journal, vol. XI, p. 216) and, like that it is produced by the collapsion of the water rushing into the vacuum left by the bubbles of steam. It does not seem easy to account for the distinct sensations in the two ears. I incline to think, that it may have arisen from the direction of the undulatory motion having been more favourable to one ear than to the other according to position, and that the ear most directly opposed to the undulation may have received a stronger action than the other. If this were the case, the sensation ought to be single when the face was directly opposed to the steam orifice.

Of the same nature as that of simmering.

W. N.

VI.

Miscellaneous Observations on some Phenomena of Combustion. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

NOT knowing whether your publication takes in such trifling observations as what I am about to offer, I leave it to your choice to insert them or not; but not having met with them in my own reading, I conceive they may be acceptable to some of your younger readers.

1. I have made at different times phosphorus bottles, to serve instead of a flint and steel (by simply melting the phosphorus in a dry and heated phial, and allowing it to burn for a few seconds); and I find, that, though kept with a well ground stopper, they always get wet after using about twelve or fifteen times, probably from the phosphoric acid attracting water from the atmosphere at the times of opening the bottle. This, beside the expense attendant on renewing them, sometimes proves a mortifying disappontment;

Phosphorus bottles.

ment; but by dipping the match as usual, and then rubbing it briskly upon a piece of old woollen cloth, or even brown paper, it will almost certainly inflame.

Enlarged flame of an Argand lamp.

2. When the leaking vessel of an Argand lamp is removed, after it has filled with oil, the lamp continuing lighted, the flame rises to the height of several inches. Long after I had seen this I was unable to account for it, but it arises no doubt from the gas being confined in the inner cylinder, which will not inflame till the admission of a current of air, for the flame does not rise unless the receiver has been full for a short time, nor does it ever rise above a certain height.

Candle rekindled without contact of flame.

3. When I first began to attend to Natural Philosophy, an eminent lecturer, a chemist, whose course I attended, gave as an instance of attraction, "A candle just blown out is relighted on the approach of a lighted body, as it attracts the flame without touch:" and it was long before I saw the absurdity of this explanation, by having recourse to the abstruse doctrine of attraction, instead of saying, the carburetted hidrogen, that is produced, inflames, and thereby creates a fresh supply of inflammable gasses.

G. O.

Camphor in carraway seed.

I find that camphor is contained in considerable proportion in the seeds of carraway *(carum carui)*. 1 lb. of seed yields about 4 oz. of oil, and ¼ oz. of camphor.

REPLY.

Remarks.

Many of the greatest discoveries have arisen from circumstances apparently trifling; for which reason they ought never to be considered as unworthy of attention. The first observation of G. O. is known to the venders of phosphorus apparatus, who generally put a piece of cork for rubbing the match upon.—I am disposed to think, that the augmentation of the flame in an Argand lamp, when the oil cup is taken off, arises merely from the increased passage for the ascending air. If the hand or any other flat surface be held beneath the lower orifice after the oil cup (as usually made) has been removed, all the variations from the brilliant white to the brown smoaky flame may be observed with great advantage, accordingly as the hand is held more or less remote from the aperture.

W. N.

VII.

On the Process for procuring the Metal of Potassium by Means of Iron. In a Letter from a Correspondent.

SIR,

WISHING to repeat some of the brilliant experiments of Mr. Davy, I endeavoured to procure the metal of potash by means of iron, in which I have repeatedly failed. As I perceive Mr. Curaudau, a French chemist, has also failed in procuring potassium in this way, I have taken the liberty to request you will communicate in your next number every information you may be possessed of with respect to the minutiæ of the process, and the best and readiest method of constructing an apparatus. I hope you will attend to this, being persuaded, that it is the difficulties that are first met with which have prevented other chemists from following the steps of Mr. Davy.

Query on the method of procuring potassium by means of iron.

I remain, Sir,
Your obedient reader,
A. B.

P. S. I understand Mr. Hachett has written something on the subject, but I have not seen an account of his experiments.

REPLY.

As the experiment never fails in the laboratory of the Royal Institution, I apprehend a brief account of the manner in which it is there performed will be the most satisfactory answer I can give to my correspondent.

A common gunbarrel, made very clean, and bent in the manner

Method described.

manner represented at fig. 6, plate IV, has an iron receptacle, A, ground into one end of it, and furnished with a ground stopper. This receptacle contains from two to three ounces of potash (pure potash) that has been in fusion. Very clean iron turnings are placed in the middle of the barrel D, which is heated to whiteness; and the potash is kept cool till this temperature is attained. The potash is then slowly fused, and it passes through a hole in the receptacle upon the turnings. The heat is kept up till gaseous matter ceases to come over. The extremity of the tube B is kept cool, and in this the potassium collects. Common air is prevented from entering by a glass tube, C, which supports the weight of a column of mercury.

VIII.

Description of a reflective Goniometer. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S**.

Crystallography much advanced.

FROM the advances that have been made of late years in crystallography, a very large proportion of mineral substances may now be recognised, if we can ascertain the angular dimensions of their external forms, or the relative position of those surfaces, that are exposed by fracture. But though the modifications of tetrahedrons, of cubes, and of those other regular solids, to which the adventitious aid of geometry could be correctly applied, have been determined with the utmost precision, yet it has been often a subject of regret, that our instruments for measuring the angles of crystals are not possessed of equal accuracy, and that in applying the goniometer to small crystals, where the radius in contact with the surface is necessarily very short, the measures, even when taken with a steady hand, will often deviate too much from the truth to aid us in determining the species to which a substance belongs.

But a good instrument for measuring the angles of crystals still wanting.

This defect supplied.

A means of remedying this defect has lately occurred to me, by which in most cases the inclination of surfaces may be

* Philos. Trans. for 1809, p. 253.

be measured as exactly as is wanted for common purposes, and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only $\frac{1}{50}$ of an inch in breadth may be determined with as much precision as those of any larger crystals.

By using a lengthened radius.

For this purpose, the ray of light reflected from the surface is employed as radius, instead of the surface itself, and accordingly for a radius of $\frac{1}{50}$ of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or for greater accuracy we may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us.

Instrument described.

The instrument which I use consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar (Plate V.) This axle, being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion.

Method of using it.

This position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye.

The circle is then set to *zero*, or 180°, by an index attached to the pillar that supports it.

The small axle is then turned till the farther surface reflects the light of a candle, or other definite object to the eye; and, lastly, (the eye being kept steadily in the same place) the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been. The angle through which the circle has moved is in fact the supplement to the inclination of the surfaces; but as the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shown by the index, without need of any computation.

Angle of an irregular fracture may be measured by it.

It may here be observed, that it is by no means necessary to have a clean uniform fracture for this application of the instrument to the structure of laminated substances; for since all those small portions of a shattered surface, that are parallel to one another (though not in the same plane), glisten at once with the same light, the angle of an irregular fracture may be determined nearly as well, as when the reflecting fragments are actually in the same plane.

Small errour from parallax.

In this method of taking the measure of an angle, when the eye and candle are only ten or twelve inches distant, a small errour may arise from parallax, if the intersection of the planes or edge of the crystal be not accurately in a line with the axis of motion *; but such an errour may be rendered insensible, even in that mode of using the instrument, by due care in placing the crystal; and when the surfaces are sufficiently smooth to reflect a distinct image of objects, all errour from the same source may be entirely obviated by another method of using it.

May be obviated.

For this purpose, if the eye be brought within about an inch of the reflecting surface, the reflected image of some distant chimney may be seen inverted beneath its true place, and by turning the small side may be brought to correspond apparently with the bottom of the house (or with some other distant horizontal line.) In this position the surface accurately bisects the angle, which the height of that house subtends at the eye (or rather at the reflecting surface); then, by turning the whole circle and crystal together, the other surface, however small, may be brought exactly into the same position; and the angle of the surfaces may thus be measured, with a degree of precision which has not hitherto been expected in goniometry.

Accuracy of the instrument.

The accuracy, indeed, of this instrument is such, that a circle of moderate dimensions, with a vernier adapted to it, will probably afford corrections to many former observations. I have already remarked one instance of a mistake that prevails respecting the common carbonate of lime, and I am induced to mention it, because this substance is very likely

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* I cannot omit mentioning, that Mr. Sowerby had thought of employing reflection for this purpose, nearly at the same time as myself; but did not succeed to his satisfaction, in consequence of an attempt to fix the position of the eye. For when the line of sight is determined by a point connected with the apparatus, the radius employed is thereby limited to the extent of the instrument, and the errour from parallax is manifestly increased.

to be employed as a test of the correctness of such a goniometer, by any one who is not convinced of its accuracy from a distinct conception of the principles of its construction.

Mistake in the angles of carbonate of lime corrected by it,

The inclination of the surfaces of a primitive crystal of carbonate of lime is stated, with great appearance of precision, to be 104° 28′ 40″: a result deduced from the supposed position of its axis at an angle of 45° with each of the surfaces, and from other seducing circumstances of apparent harmony by simple ratios. But however strong the presumption might be that this angle, which by measurement approaches to 45°, is actually so, it must nevertheless be in fact about 45° 20′; for I find the inclination of the surfaces to each other is very nearly, if not accurately 105°, as it was formerly determined to be by Huygens*; and since the measure of the superficial angle given by Sir Isaac Newton† corresponds with this determination of Huygens, his evidence may be considered as a further confirmation of the same result; for it may be presumed, that he would not adopt the measures of others, without a careful examination.

and made to agree with the measures of Huygens and Newton.

Explanation of the plate.

In the annexed plate,

a b. Is the principal circle of the goniometer graduated on its edge.

c c. The axle of the circle.

d. A milled head by which the circle is turned.

e e. The small axle for turning the crystal, without moving the circle.

f. A milled head on the small axle.

g. A brass plate supported by the pillar, and graduated as a vernier to every five minutes.

h. The extremity of a small spring, by which the circle is stopped at 180°, without the trouble of reading off.

i i and *k k*. Are two centres of motion, the one horizontal, the other vertical, for adjusting the position of a crystal: one turned by the handle *l*, the other by the milled head *m*.

The crystal being attached to a screw-head at the point *n*

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* Huygenii Opera Reliqua, Tom. I, p. 73—Tract. de Lumine.

† Newton's Optics, 8vo, p. 329, Qu. 25, concerning Iceland Crystal.

(in the centre of all the motions), with one of its surfaces as nearly parallel as may be to the milled head *m*, is next rendered truly parallel to the axis by turning the handle *l* till the reflected image of a horizontal line is seen to be horizontal.

By means of the milled head *f*, the second surface is then brought into the position of the first, and if the reflected image from this surface is found not to be horizontal, it is rendered so by turning the milled head *m*, and since this motion is parallel to the first surface, it does not derange the preceding adjustment.

IX.

Remarks upon Meteorology; with a Specimen of a new Meteorological Table. By J. Bostock, *M.D.*

Predictive meteorology of great use.

THAT part of the science of meteorology, which consists in predicting the changes of the weather, is not only interesting from its connection with the great operations of nature, but is also in itself of obvious practical utility. It has accordingly engaged the attention of many philosophers; and numerous attempts, some derived from hypothesis, and others founded upon supposed experience, have been formed, to lay down a set of rules, which might apply to the various circumstances that present themselves. The basis of these rules must be observation; and it is from a conviction of this truth, that many scientific men, in different parts of the world, have assiduously employed themselves in forming what are called meteorological tables. But, I think, I may venture to assert, that the benefit derived from these tables of observations by no means corresponds with the trouble that has been bestowed upon them. After marking down the height of the barometer and thermometer, noting the direction and force of the wind, and measuring the quantity of rain, for years together, it does not appear, that the observer is in any degree enabled to determine, whether the next day will be fair or foul, calm or stormy. On the contrary

It must be founded on observation.

The benefit arising from meteorological tables not yet answerable to their trouble.

trary we know, that there are many persons who are ignorant of all the principles of science, and have never systematically studied the subject, who have notwithstanding arrived at that practical knowledge, which gives the study its principal value. I now refer to farmers, sailors, and such other persons as are led by their occupations to spend much of their time in the open air; and to whom a knowledge of the changes of the weather is of immediate importance. Every one who has conversed much with these persons must know, that they have acquired great sagacity in predicting these changes; and yet it unfortunately happens, that their knowledge is of little use to any one but themselves, not being in the habit of conversing upon the subject, and having acquired their ideas solely by the exercise of their own faculties, they are often absolutely unable to convey to any one else the reasons by which their own judgment has been determined. It is now some years since I became impressed with this view of the subject, and since I resolved to endeavour to imitate the method by which this practical knowledge appears to be obtained, and at the same time to frame a nomenclature, by which I might be able to convey my ideas to others. Although my mode of life, as a resident in the middle of a large town, has been unfavourable to such pursuits, yet I think that I have been not altogether unsuccessful in my attempt; some points of importance I consider myself as having ascertained, and many more have suggested themselves, which a longer series of observations must confirm or refute.

More has been done without the advantage of science.

But this knowledge is confined to the individual.

These two modes of attaining knowledge might be combined.

My plan of observation, as it appears, essentially consists, first, in paying a constant attention to atmospherical phenomena of all kinds; and secondly, in adopting a nomenclature, by which these phenomena may be accurately recorded, and by which the observations made at different times, and in different places, may be compared together. In the meteorological journals which have been hitherto published, either by learned societies, or by individuals, we have had the height of the barometer and thermometer taken at two or three stated periods in the course of the day, the degree of the hygrometer is frequently added, the quantity of rain that has fallen, the force and direction of the wind, and

Attempt to effect this.

Usual meteorological tables.

and occasionally there are subjoined to these a few very general remarks, as to the cloudiness or brightness of the atmosphere, the presence of fogs, mists, &c.* It is no doubt necessary and desirable to ascertain these points; but they are very far from being the most important objects of attention. What I have been in the habit of considering as such are first the nature and shape of the clouds, and the progressive changes which they are undergoing; secondly, the relative state of the barometer, whether rising or falling, and with what degree of rapidity; and thirdly, the relative state of the wind, whether increasing or diminishing, whether it has lately changed its direction, and from what point it proceeded before its change. These are the objects to which the attention should be uniformly directed, and which may be considered as the basis of all meteorological predictions. There are also a number of accessary circumstances, that may be occasionally employed. Of these some of the most important are, any sudden or remarkable changes in the temperature or humidity of the atmosphere; the aspect of mountains or other distant objects; the appearance of the horizon as contrasted with that of the higher regions of the atmosphere; the state of the air as to fogs, mists, dew, haze, &c.; the electric state of the air; the manner in which smoke is affected in passing from chimney tops; and the state of evaporation from the surface of the earth. It will be evident, that to enter into a full description of all the modifications and varieties of these different phenomena would require a long dissertation; my present object is however merely to offer a specimen of my diary, and to accompany it with such explanations as may render it intelligible, and at the same time illustrate its principles.

Most important objects in meteorology.

Other circumstances to be noticed.

Specimen of a diary.

I have selected as a specimen of my diary that part of it, which contains an account of the weather in September last, as it was marked by many striking changes, which will give me an opportunity of explaining several different parts of my system. I must observe, that neither this, nor any other part of my diary, is in what I consider as a perfect state; my observations were necessarily interrupted from a variety of causes, and it is impossible, that any one not residing in the country, and being much in the open air, could pay that constant attention to atmospherical phenomena, which I regard as so important. I may premise, that the preceding month of August, although wet, was remarkable rather for the frequency, than the long continuance of the rains; it was a bad harvest month, but might have passed for seasonable spring weather; the temperature was low, but uniform. Towards the end of the month there were some warmer and drier days, and the farmers began to hope that a fine autumn was commencing; the event however proved extremely contrary to their expectations.

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* See the Journal published by the Royal Society, that in the Journal de Physique, that in the Bibliotheque Britannique, Bent's Meteorological Tables, the table published in this work, in the Athenæum, and in the Philosophical, Monthly, and Gentleman's Magazines.

Weather of August 1809.

SEPTEMBER, 1809.

1	SE_{2}	29·72—	Partially clear, some light clouds, nearly calm. | Continued all the day nearly clear and calm with a gentle
	SE_{2}	29·65—	breeze. | Evening clear and calm; a very pleasant day. Mountains clear, but light. No tufts or lines, but a few small, round, dark clouds; afternoon transparent.
2	SE_{3}	29·60—	Uniformly cloudy, gentle breeze. | Afterwards some breaks, the breeze increased. At noon some rain came on for 2 hours. Afternoon cleared partially, and wind went to E. Then some
	E_{4}	29·50—	heavy broken clouds, and an imperfect arc between E and W. | Evening wind high, partially clear, driving showers in the night.
3	E_{3}	29·48	Fresh breeze, heavy clouds, tendency to rain. | Forenoon more clear and less windy. Afternoon a heavy shower, then partially clear and a gentle breeze. |
	E_{2}	29·50+	Evening some lightning. The state of the atmosphere, which produced the squall

			squall of the 2d, now changed, but wind still E.
4	E E	29·53 29·53	Partially clear, breeze, dense clouds. | Forenoon partially clear, warm, pleasant, gentle breeze. Then became cloudy and continued so the rest of the day, nearly calm. Tendency to rain in the afternoon. In the forenoon a precipitating arc S to N, in the afternoon went to SE. Small contiguous solar halo; clouds moved rapidly from SE. | Evening dark, calm, uniformly cloudy.
5	NE	29·45— 29·41	Uniformly cloudy and calm. | Continued all the day uniformly cloudy, except a short time about noon. Almost quite calm, wind went qsite round to W, S, and again to NE. Afternoon rather foggy. Evening dark and calm.
6	 NNW	29·41— 29·28—	Rain in the night, cloudy, calm. | Grew partially clear about noon, and at 3 P. M. a gentle breeze sprung up from NNW. Pleasant, but close day. Evening a good deal of lightning and some heavy showers.
7	ENE E	29·23— 29·16 29·20+	Cloudy with some breaks, nearly calm. Forenoon grew partially clear, and breeze increased. At noon heavy showers; then partially clear, gentle breeze, lightning in the afternoon. | Evening bright, almost perfectly clear, gentle breeze. The barometer, which had been falling almost uniformly for the last seven days, began to rise about 6 P. M.
8	E	29·30+	Partially clear, calm. | Continued calm during the greatest part of the day; a gentle breeze from NNW in the afternoon. In the forenoon there was a precipitating arc from ESE. Gradually grew

grew more clear, and in the afternoon
nearly so; dissolving constitution; plea-
NNW 29·38+ sant day. | Evening bright. Lightning
2 in the night.
9 W 29·49+ Cloudy, thick body of clouds moving
4 before the wind; breeze. | A tendency
to clear early in the forenoon, but again
became cloudy, and about noon a little
NW 29·62+ rain. Afternoon round shaded body of
5 clouds moving before the wind. Cold,
irregular, fresh breeze. Large piled
clouds at sun-set. Sour day.
16 S 29·56— Bright, nearly clear, gentle breeze.
3 Some lines from W to E. | Large roll-
ing clouds began to form about 9, gra-
dually grew more cloudy; at 12 it be-
gan to rain and rained for 2 or 3 hours.
W 29·55 About 3 P. M. wind went to W, at
1 first a breeze, then grew more calm.
Became more clear. | Evening very
clear and calm. In the afternoon an
imperfect wreathed arc from NW.
11 W 29·58+ Cloudy with breaks, breeze. | Conti-
4 nued so during the day, generally bright
with large clouds, sometimes rounded
and rolling, sometimes heavy and bro-
W 29·66+ ken. Cold breeze. | Evening cloudy,
3 breeze.
12 W 29·68+ Bright, partially clear, cool breeze.
4 Clouds rather broken, moving before
the wind. | Continued all the day bright,
some heavy clouds, breeze, inclined to
WSW 29·70 squally. | Evening bright, clear, with
2 some heavy clouds; nearly calm. At
sunset wind went more towards S. This
morning seemed like a clearing day,
but at sunset the mountains were trans-
parent.
13 SSE 29·69— Partially clear, gentle breeze. | Clouds
3 increased in the forenoon, and at noon
uniformly

	SSE 3	29·56—	uniformly cloudy. Heavy and gray atmosphere, shaded towards the horizon. About 3 began to rain, and continued more or less all the afternoon and evening. Cold and fresh breeze.
14	SE 3	29·52	Partially clear, gentle breeze. | In the forenoon irregular tufts, generally pointing to W. Shower about noon. At 1 wind went to E, clouds dense and broken, now more clear.
	ENE 2	29·74+	| Evening very clear, starlight, gentle breeze; in the afternoon irregular tufts tending to N.
15	NE 1	29·92+	Partially clear, calm, veering. | In the forenoon dense clouds. In the afternoon an arc from NNW, with some flocks pointing to E. Gentle breeze from NNW.
	NNW 2	30·02	| Evening cloudy, nearly calm.
16	S 2	29·92—	Uniformly cloudy, nearly calm. | Misty rain came on at 8, and continued for 2 hours. About 10 wind went to W, and became a fresh and squally breeze; tendency to clear at noon. Mountains black and large; a low stratum of clouds moving rapidly from SW. In the afternoon thick rain, squally breeze.
	W 3.3	29·85	Evening partially clear and more calm.
17	W 4	29·85	Large heavy clouds slowly moving before the wind. | Showers during the day, clouds with some breaks, breeze.
	W 2	29·85	In the afternoon cleared, and the evening was bright and nearly calm.
18	SSE 2	29·65	Seemed to have rained much in the night, now heavy rain, uniformly cloudy, nearly calm, clouds moving rather rapidly from SE. | Rain continued until noon, then showers. Wind went to W, barometer continued falling rapidly. Partially clear, large clouds floating on

			on a gray ground. Wind squally in
	W$_{4,6,7}$	29.26+	the afternoon; about 8 became high, and at 10½ grew a violent storm; barometer began to rise about 10.
19	WNW	29·56+	Some squally showers in the night, still windy, partially clear, clouds moving from NW. | In the forenoon heavy showers. About noon became more
	WNW$_2$	29·63+	clear and calm; afternoon bright, gentle breeze. | Evening nearly calm, at first nearly clear, then more cloudy.
20	SE$_2$	29·27—	Heavy rain, uniformly cloudy, nearly calm. | In the forenoon wind went to W, breeze, partially clear, with occasional heavy showers. | In the evening
	W	29·09+	the wind went to W, became high with squally showers. Barometer had fallen ·54 in 24 hours, now tends to rise.
21	W	29·53+	Still high wind; bright, partially clear, large dense, shaded clouds, moving before the wind. | Continued bright with large clouds, breeze. Barometer had risen ·44 in 12 hours, but about noon
	SSW$_3$	29·58—	tended to fall. Afternoon lines pointing to W; then grew uniformly cloudy and precipitating, gentle breeze.
22	S$_3$	29·33+	Rain in the night, and wind. Now rain, uniformly dull, breeze. | About 10 cleared partially with a breeze; at
	SW$_5$	29·43	noon went to SW; during the rest of the day sometimes bright, sometimes considerably clouded, precipitating.
	S$_2$	29·40—	Wind returned to S in the evening. About noon there were many irregular tufts, the ends pointing to SE; when the wind went to SW the tufts all disappeared, and white rolling clouds formed. In the afternoon an imperfect arc from SW.

Rain

23	WSW 2	29·32+	Rain in the night; partially clear, clouds slowly moving from SW. | Continued so until noon, then it grew more cloudy, and then a heavy shower. Wind
	W 6·2	29·55+	went to W and was high for some hours. A heavy shower between 4 and 5 P.M. Then became more clear and calm, large clouds moving before the wind; gentle breeze. Lightning.
24		29·64+	Very heavy showers in the morning, calm. | For some time partially clear;
		29·67	then heavy rain for a considerable part of the forenoon. Large shaded clouds,
	SW 2	29·64—	calm. Afternoon gentle breeze, pleasant. | Evening more cloudy with showers, nearly calm. Lightning.
25	W 2	29·56+	Rain in the night, still showery; partially clear, broken clouds, breeze. |
	WNW 2	29·73+	In the afternoon some heavy rain, breeze. In the forenoon an imperfect precipitating arc from SW, with rolling clouds moving before the wind; mountains clear, not dark. | Evening bright; round clouds slowly moving before the wind, dissolving; nearly calm.
26	W 3	29·81	Rain in the night; partially clear, clouds heavy and broken, moving slowly before the wind. | During the day irregular
	SW 4	29·50—	breeze, pleasant. About noon long, irregular wreathed arc from NNW to SSE, some crossing lines pointing to SW. Afternoon more cloudy, precipitating sky, a little rain, irregular breeze.
27	W 4·6	29·16—	Rain in the night, which still continues; uniformly cloudy with breeze. | Forenoon wind rose and for some hours was very high, with violent showers of rain
	NNW 4	29·55+	and hail. About 4 wind abated, went to N, atmosphere cleared, and became dissolving.

			dissolving. | Evening cool, fresh breeze from NNW; bright with some dense clouds.
28	NW 6 NW 5	29·68+ 29·83+	Windy in the night, still continues so; partially clear, dense and broken clouds moving before the wind. | During the day cold fresh breeze; partially clear, large shaded clouds, some flying showers. | Evening more calm and clear.
29	NNW 3 W 2 S 1	29·85 29·82—	Partially clear, dense clouds moving slowly before the wind. | During the day sometimes bright, partially clear; Forenoon feathered arc from N; lower stratum of clouds moving slowly from W; muddy with dark clouds in N. | Evening cloudy, calm. Reverse resolving day.
30	S 2 W 4 NNW 2	29·70+ 29·89+	Uniformly cloudy, nearly calm. | Mild, misty rain for 2 or 3 hours in the forenoon; about noon wind went to W, fresh breeze sprung up and it cleared; wind then went to NW, grew bright, clear, and nearly calm. | Evening brilliant, a few clouds, nearly calm, dissolving.

Diary explained.

Wind.

The diary consists of two columns, the first for the direction and force of the wind, the second for the height of the barometer, and to these is appended a large space for occasional remarks. For the direction of the wind I think it sufficiently minute to divide the compass into 16 points; N, NNE, NE, ENE, E, &c. for the other quarters. The force of the wind is comprehended under 8 general terms, which have each their appropriate figure. 1, calm; 2, nearly calm; 3, gentle breeze; 4, breeze; 5, fresh breeze; 6, windy; 7, high wind; 8, violent storm. There are some other circumstances connected with the wind, whether the force be uniform, or whether it be irregular, what is usually called squally; whether it be increasing or diminishing at the time of the observation; whether it blow steadily from one point, or veer about; whether it shift irregularly or revolve gradually; and lastly, whether it appear to extend high into the atmosphere, or only affect the lower stratum.

Barometer. The height of the barometer is noticed between 8 and 9 in the morning, and at the same hour in the evening; but its absolute height is of little consequence, unless we know whether it be rising or falling, when it began to rise or fall, and also what connection there is between these circumstances and the direction of the wind, and the shape and appearance of the clouds. The following case will illustrate the greater importance of *relative* than of *absolute* observations upon the subject. Suppose that nothing was known of the state of the preceding day, but in the morning we find the barometer at 29·75, its medium height, the wind W, a gentle breeze, and the sky partially clear. These circumstances afford no decisive indication of the probable state of the day, and according to the preceding appearances, they may afford either a favourable or an unfavourable prognostic. If the evening before the wind had been NW, the atmosphere brilliant, and the barometer 29·80, we may expect, that the wind is travelling to the SW, and that rain will probably ensue; but, on the contrary, if the preceding day had been rainy, the wind in a southerly point, and the barometer low, we may fairly hope that an improvement will take place in the weather. It is in my opinion principally for want of this plan of retrospective observation, that our meteorological instruments are of so little use.

Clouds. The occasional remarks form by far the most important part of the above table, and upon them I shall now proceed to make some comments. The phenomena of the clouds I consider as forming the most important part of my system of observation, and they require a great variety of terms to designate all their characters. They are varied as to their general shape, size, colour, density, height from the surface of the Earth, and motion. Beside these there are three points particularly to be attended to, whether their bulk be increasing or diminishing; whether any change be taking place in their character, and one kind of cloud be gradually

dually passing, or becoming converted into another species; and what is their relation with respect to the state and direction of the wind. To enter into a full detail of this subject would alone occupy a long dissertation, I shall merely take occasion to illustrate some parts of it in the explanation which I shall give of the specimen of my journal. In the account of the state of the weather on the 1st of September I have mentioned the appearance of distant objects. Those to which I refer are the mountains, which are seen in different directions from the vicinity of Liverpool. To the W and SW we have a long line of hills situate in the counties of Flint and Denbigh, from 20 to 30 miles distant, and beyond these the summits of some of the Caernarvonshire mountains are visible. To the S we have the high lands in the SW of Cheshire, and beyond these are occasionally seen the peaks of the Montgomeryshire mountains. To the E and SE is the range of hills that separates the counties of York and Lancaster, and also those in the N of Derbyshire and Staffordshire. It is not in many situations, that the spectator has an opportunity of seeing all these objects from one point of view, nor is it often that the state of the atmosphere renders them all visible at any one time; but it will be perceived, that our situation is very favourable for observations of this kind. Distant objects vary as to their apparent size, their distinctness, and their colour.

Appearance of distant objects.

Under the title of particular states of the atmosphere I mean to designate some phenomena, which are sufficiently obvious and characteristic, but the immediate cause of which is doubtful. I have observed in my notes, that the afternoon of the 1st of September was transparent, and I shall endeavour to describe what I mean to designate by this term. A transparent condition of the atmosphere seldom lasts for more than a few hours, and these generally before sunset. The sky is either clear, or if there be any clouds, they consist of fine lines lying parallel to each other, or of irregular tufts in the upper parts of the sky, or of small, round, spotted clouds near the horizon. What however gives the character and name to this kind of day is the peculiarly beautiful appearance, which moderately distant objects

State of the atmosphere.

objects present, especially any large expanse of water. I cannot describe it better than by saying it is like a picture very highly varnished. Another characteristic circumstance in a transparent atmosphere is the appearance of smoke as it ascends from a chimney; it mounts up for a considerable height in nearly a straight line, and in a slender column, and is a much longer time than usual in diffusing itself through the air. The sun frequently sets with a degree of mist, but often surrounded by the most brilliant colours, and for some time after its orb has disappeared, the whole horizon exhibits an extremely delicate lilac or violet tinge.

Sign of rain.

Every one who has not been in the habit of minutely attending to the states of the atmosphere is ready to exclaim, that the weather has every appearance of being settled, and that we are at least certain of the following day being fine. My observations however lead me to the conclusion, that there is no more infallible sign of a change of weather, than one of these transparent evenings.

Liverpool, Feb. 5, 1810.

(To be continued.)

X.

On the Nature of the intervertebral Substance in Fish and Quadrupeds. By EVERARD HOME, *Esq. F. R. S.*[*]

Peculiarity in the spine of the shark.

IN examining the internal structure of a squalus maximus of Linnæus, that lately came under my observation, a description of which will be the subject of a future paper, I met with a peculiarity in the intervertebral substance of the spine not hitherto made known to the public.

Fluid in the intervertebral substance.

The fish is thirty feet six inches long, the diameter of the larger vertebræ near the head, seven inches. The intervertebral substance was cut into by Mr. Clift four days after the fish was brought on shore, and a limpid fluid rushed out with so much velocity, that it rose to the height of four feet.

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[*] Philos. Trans. for 1809, p. 177.

Coagulates after death.

At the end of twelve days, I had an opportunity of examining a portion of the spine, the intervertebral joints of which were preserved entire; upon sawing through two of the vertebræ, a fluid was met with, of the consistence of liquid jelly with clots of different sizes floating in it, so that in eight days a considerable tendency to coagulation had taken place, although the fluid was entirely excluded from the external air.

Cavity containing it.

The form of the cavity, thus exposed by a longitudinal section being made of it, is nearly spherical, capable of containing three pints of liquid; the lateral parts are ligamentous and elastic, uniting together the edges of the concave surfaces of the two contiguous vertebræ. When the liquid is evacuated, the elasticity of the lateral ligaments brings the ends of the vertebræ within an inch and half of each other; in this state the inner layers of the ligaments, which are less firm in texture than the outer, project into the cavity, and may be mistaken for a part of its natural contents; this portion when soaked in water swells out to a considerable size, the water readily insinuating itself between the membranous layers of which it is composed.

Lateral ligaments.

The whole thickness of the lateral ligaments is about one inch, the external half of which is compact and elastic, the other appears to possess a very slight degree of elasticity. The appearance of the joint is shown in the annexed drawing, and an account of the analysis of the fluid by Mr. W. Brande forms a postscript to this paper. Every part of the mechanism is formed on so large a scale, that it is rendered conspicuous, and nothing is left to doubt or conjecture; the nature of the joint is different from every other that is met with in animal bodies, and there are many circumstances respecting it, which render it uncertain whether human ingenuity can ever make any resemblance to it, that can be applied to the purposes of mechanics.

The joint enables us to explain the common principles of the structure of the spine.

These would have been sufficient grounds for bringing this subject before the Society; but there are others of still greater importance, which have induced me to make it a separate communication; it enables us to explain the general principle upon which all intervertebral joints are formed, which has been hitherto but imperfectly understood. With this view, I will first describe the principle upon which this particular joint is formed, and then show the resemblance that it bears to those of other animals, in which the parts are not so readily distinguished from one another, and consequently their precise use has not been accurately ascertained.

Use of the fluid.

The fluid contained in the cavity, being incompressible, preserves a proper interval between the vertebræ, to allow of the play of the lateral elastic ligaments, and forms a ball round which the concave surfaces of the vertebræ are moved, and readily adapts itself to every change, which takes place in the form of the cavity.

Elasticity of the ligaments,

The elasticity of the ligaments, by its constant action renders the joint always firm, independent of any other support, and keeps the ends of the vertebræ opposed to each other, so that the whole spine is preserved in a straight line, unless it is acted on by muscles or some other power. When a muscular force is applied to one side of the spine, it stretches the elastic ligament on the opposite side of the joint, and as soon as that force ceases to act, the joint returns to the former state. This is one of the most beautiful instances in nature of elasticity being employed as a substitute for muscular action.

a substitute for muscular action.

The extent of the motion in each particular joint is undoubtedly small, but this is compensated by their number, and the elasticity of the vertebræ themselves.

Other fish have a similar fluid.

Fish in general have their vertebræ formed with similar concavities to those of the squalus maximus; these, when examined after death, contain a solid jelly, but in the living fish it is found in a fluid state. This fact was acertained in the skate, the smallness of the quantity of fluid in any one joint, and the readiness with which it coagulates after death, prevented it from being before observed: the fluid in the skate is found by Mr. Brande to have the same properties, as far as the small quantity that can be collected admits of examination, with that in the squalus maximus.

Form of the cavity differs

Although this structure of the intervertebral joint appears to be common to fish in general; the form of the cavity is not in all exactly the same; in the skate it is very similar to that in the squali, but in the common eel it is more oblong, the longitudinal diameter being about one third longer than the transverse one..

It it evidently contrived for producing the quick vibratory lateral motion, which is peculiar to the back bones of fish while swimming, and enables them to continue that motion for a great length of time, with a small degree of muscular action.

Peculiarity in the spine of the sturgeon,

In the sturgeon, there are some curious peculiarities in the structure of the spine. Externally there is the common appearance of regular vertebræ, but these prove to be only cartilaginous rings, the edges of which are nearly in contact, and are united together by elastic ligaments, forming a tube the whole length of the spine; this is lined throughout its internal surface with a firm compact elastic substance, about the thickness of a cartilaginous tube; within this is a soft flexible substance in a small degree elastic; in the centre there is a chain of cavities in the form of lozenges, containing a fluid, and communicating with one another by very small apertures bearing a slight similarity to the intervertebral cavities of the spine in other fish.

and the lamprey.

As all the different parts of which this spine is composed are more or less elastic, except the central fluid, it must have great flexibility adapting it to the motions of this particular fish. The structure of the spine in the lamprey eel resembles that of the sturgeon.

Spine of the whale similar to that of quadrupeds.

The intervertebral joint, which is common to fish, is not met with in any of the whale tribe, whose motion through the water is principally effected by means of their horizontal tail; in them the substance employed to unite the vertebræ together is the same as in quadrupeds in general, and from the size of the vertebræ it is on a larger scale, and rendered more conspicuous.

A gelatinous substance instead of the fluid.

The external portion is very firm and compact, is ranged in concentric circles with transverse fibres uniting the layers together, it becomes softer towards the middle, and in the centre there is a pliant soft substance without elasticity, but admitting of extension more like a jelly than an organized body, corresponding in its use to the incompressible fluid in the fish.

Other animals examined.

Hog & rabbit.

To ascertain whether this structure was generally met with in the spines of quadrupeds, Mr. Brodie, at my request, examined the intervertebral substance in a great many animals, and found, what, undoubtedly, was very little to be expected, that in the hog and rabbit, in the central part, there is a cavity with a smooth internal surface of the extent of half the diameter of the vertebra, in which is contained a thick gelatinous fluid, so that in some quadrupeds there is an approach towards the intervertebral joint in fish; but whether this is to answer any essential purpose to these animals, or is only to form an intermediate link in the chain of gradation of structures, which is so uniformly adhered to in the productions of nature, cannot at present be determined.

Bullock, sheep deer, monkey, and man.

Allegator.

Snake.

In the bullock, sheep, deer, monkey, and man, the structure corresponds with that of the whale; in the three last, the central substance appears to be the most compact. Besides the structures already mentioned, there is in some animals one of a very different kind; in the alligator the vertebræ through the whole length of the spine have regular joints between them, the surfaces are covered with articulating cartilages; and there is synovia and a capsular ligament. In the snake, there is a regular ball and socket joint between every two vertebræ; so that the means employed for the motion of the back bone in different animals, comprehends almost every species of joint with which we are acquainted.

Having mentioned a sufficient number of facts to point out the animals, in which the different structures of the intervertebral substance are to be found, I have abstained from being more particular in my account; as it would in no respect elucidate the principal object of the present communication.

Human spine.

From the facts and observations which have been stated, it appears that the intervertebral substance of the human spine does not consist entirely of elastic ligament, dense in its texture at the circumference, and becoming gradually softer towards the centre; but that the middle portion is composed of materials which render it very pliant, though not at all elastic, fitting it to keep the vertebræ at the proper distance from each other, so as to admit of the action of the lateral elastic ligaments.

Curvature of the spine.

When this knowledge is applied to the treatment of curvatures of the spine, a complaint so commonly met with in young women, whose strength does not bear the necessary proportion to the growth of the body, it will show the great impropriety of overstretching the intervertebral ligaments, since in that state the central substance no longer supports the vertebræ, and the joints must lose their proper firmness, which will be attended with many disadvantages.

Advantages of comparative anatomy.

As the principal motive, which induces me to prosecute the laborious researches of comparative anatomy, is to attain a more complete knowledge of the structnre and functions of the human body, than can be acquired in any other way; and to apply this knowledge to the most useful of all purposes, the cure of dieases; the success which has attended my labours, in the present instance, affords me particular satisfaction; it encourages me in the pursuit of those inquiries, and holds out an invitation to others, by showing them that the paths of nature, however frequently they have been traced, are not yet sufficiently explored.

Explanation of the Plate.

Explanation of the plate.

A longitudinal section of one of the intervertebral joints of the squalus maximus, after the fluid had been evacuated, and the parts had been steeped in water.

Pl. V, *a a a a*. The section of the vertebra, to show its shape and the two concave surfaces which form the intervertebral cavities. The vertebra itself is partly bone, and partly transparent cartilage; the bony portion forms the two cup-like cavities, and the intermediate substance consists of bony cells in form of lozenges filled with cartilage.

The cavity of the joint is in its contracted state, and the inner portion of the lateral ligaments, which is made up of thin layers of a loose texture, has its interstices loaded with water, which makes it project into the cavity of the joint more than it could do in a natural state.

"The external portion of the ligament, to the thickness o half an inch, is the only truly elastic part on which it strength depends."

A chemical Analysis of the Fluid contained in the interverte-bral Cavity of the Squalus Maximus. By Mr. WILLIA BRANDE.

Analysis of the intervertebral fluid of the spine.

The fluid found in the intervertebral cavities is of an opa colour; it is semitransparent, and has a strong fishy smel and taste.

Its specific gravity is 1·027.

In the first instance it does not readily mix with water but is easily diffused through that fluid by ignition.

When heated in a water bath to a temperature of 212°, i becomes more transparent, but undergoes no farther apparent change.

Infusions of galls and of catechu produce no alteration in it.

Solution of oximuriate of mercury occasioned a very copious white precipitate, and a similar effect was produced by a solution of nitro-muriate of tin.

Nitrate of silver and acetate of lead threw down precipitates of muriate of silver and of lead.

Muriatic acid occasioned a slight cloud after two hours had elapsed, and after twenty-four hours, a small quantity of white flaky matter separated.

Alcohol produced no change.

The fluid readily mixed with a solution of pure potash, a small quantity of ammonia being at the same time evolved. Muriatic acid did not produce any immediate precipitation in this alkaline solution.

The effect of these reagents evidently proves the non-existence of gelatine in this fluid; it would also appear, that it contains no albumen, unless the effects produced by muriatic acid, and by the oximuriate of mercury and of tin, be regarded as indications of that substance.

It seems to approach nearer to *mucus* or *mucilage*, than to any other animal fluid*. When

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* By *mucus of animals*, I mean a glary fluid, which does not mix readily

Analysis of the intervertebral fluid of the shark.

When the fluid is evaporated in a temperature not exceeding 220° to half its bulk, an opaque substance, in the form of blueish white filaments, gradually separates. A thin semitransparent pellicle forms at the same time upon the surface, which, when removed, is soon succeeded by another. These pellicles were dried on bibulous paper.

The fluid part, remaining after the separation of the filamentous substance and pellicles, afforded a very distinct yellowish cloud with solutions containing tannin. It was somewhat turbid, but did not form any deposit. In other respects, it nearly resembled the original fluid before evaporation.

The filaments, which appeared during evaporation, were separated by passing the fluid through a piece of fine muslin. They resembled albumen imperfectly coagulated, not only in appearance, but in most of their chemical properties.

When the fluid began to putrify, a considerable quantity of the same substance separated spontaneously.

This substance was insoluble in water, and when boiled for a few minutes in that fluid, it became whiter, harder, and more opaque.

It underwent the same change in alcohol, and when boiled in alcohol, or in dilute muriatic acid, it became still more firm, and appeared like perfectly coagulated albumen.

In this state it was soluble in a solution of pure potash, forming a saponaceous compound, which was decomposed by dilute muriatic acid, a white flaky precipitate being formed. It possessed the other properties, which Mr. Hatchett has enumerated as belonging to coagulated albumen *.

When the pellicle, which had formed on the surface of the fluid during evaporation, was nearly dry, it became somewhat tough and elastic; it was semitransparent, and of a dirty white colour.

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dily with water, which is coagulated neither by heat nor acids, and which does not form a precipitate with solutions containing tannin.

† Vide Phil. Trans. 1800.

When

Analysis of the intervertebral fluid of the shark.

When boiled for some time in water, about three fourths of it were found to be soluble in that fluid, the remainder, when separated by filtration, possessed the properties of the albuminous substance already mentioned.

The solution afforded a copious precipitate with solutions containing tannin. It was not at first affected, either by oximuriate of mercury, or of tin; but after twenty-four hours, a slight deposit took place.

Although these reagents indicated the presence of a substance having the properties of pure gelatine in solution, yet it could not be brought to gelatinize by the usual method of evaporation.

From these experiments it would appear, that the intervertebral fluid is of a peculiar nature; that in its original properties it resembles mucus, but that under certain circumstances it is capable of being converted into modifications of gelatine and albumen.

The intervertebral fluid of the skate was found to resemble mucus; it did not exhibit any traces of albumen, but the quantity which I procured for examination being very small, I was unable to ascertain its further analogies to the fluid found in the intervertebral cavities of the squalus maximus.

XI.

On expectorated Matter. *By* GEORGE PEARSON, *M. D. F. R. S.*[*]

States of the fluids too much neglected from the humoral pathology having fallen into disrepute.

THE attention of physiologists has been very much withdrawn, for the last half century, from the consideration of the different states of the circulating and secreted fluids, in consequence of the opinion, that the nervous and fibrous or muscular systems can afford satisfactory interpretations of the phenomena of living beings; and on account of the disgust produced by the visionary properties and groundless hypotheses, originating in the humoral doctrines of Galen.

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[*] Phil. Trans. for 1809, p. 313.

But late experiments have manifested, that various things taken into the stomach can be made at pleasure to produce considerable effects, by impregnating sensibly the blood and urine, as well as the milk, sweat, and perhaps saliva. Farther; the fine experiments of professor Colman have shown, that the contagious glanders may be excited in the ass by the transfusion of the blood of a glandered horse, and the matter from the nose of the glandered ass can produce this disease in the horse or the ass*. Hence I apprehend it is reasonable to expect, that the farther investigation of the properties of the animal fluids will afford gratifying instruction to the researcher in natural science, and important practical information to the physician.

Instance of the importance of their effects.

On the present occasion, I desire the honour of communicating the knowledge I may have acquired by investigating the properties of expectorated matter secreted by the bronchial membrane. The appearances of this substance serve to regulate the judgment of the physician concerning several diseases of the lungs; but especially of that of pulmonary tubercles, which yearly destroys from 120000 to 140000 subjects of the United Kingdom. It is fit that I remark, that I do not notice in this paper the ingenious experiments of several learned chemists, because by so doing I should be led into a detail of too great extent for my design.

Importance of a knowledge of expectorated matter.

The numerous varieties of expectorated matter, according to my observation, may be arranged and characterized under the following seven heads:

Its varieties.

I. The jellylike semitransparent kind of a blueish hue, excreted in the healthy state.

II. The thin mucilagelike transparent matter, so copiously expectorated in bronchial catarrhs.

III. The thick opaque straw coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles.

IV. Puriform matter secreted without any division of continuity,

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* Mr. Colman alleges, that there is not a sufficient quantity of blood, in a single glandered ass, to excite the glanders by the transfusion of blood into the horse.

tinuity, or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions.

V. The matter which consists of opaque viscid masses, together with transparent fluid; or the second sort above stated, with nodules of the third or fourth kind.

VI. Pus from the vomicæ of tubercles.

VII. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

Other kinds.

Other kinds of matter are occasionally coughed up, such as calculi—masses of self-coagulated lymph—serous fluid—blood itself—and perhaps the vascular substance of the lungs; but I do not write on these matters, because they either do not belong to any particular recognized disease, or they are rare occurrences in some well known disease, and are too obvious to require description.

SECT. I. *Sensible, or obvious Properties.*

Jellylike bluish matter.

I. *The jellylike matter*, as already said, is excreted in the best health, as well as sometimes in disease. It is mostly coughed, or hawked up, in a morning, soon after a night's repose, during which it seems to accumulate. A few masses, or nodules, then appear of the consistence of jelly, and from the size of a pea to a hazle nut. It is also at any time liable to be excreted, in consequence of various extraneous matters irritating the fauces, to the amount of a few nodules. It is of a grayish colour, or inclining to blue, with black specks; and it is rarely whitish in nodules. The consistence is that of jelly, but of much greater tenacity. It has a barely perceivable taste of common salt, or muriate of soda. It commonly floats on water, but by agitation to disengage air bubbles, it sinks. It has no smell. To the naked eye, or assisted by a single magnifier, this matter seldom appears uniform, but consists of a mixture of opaque and transparent masses of irregular figures. With the compound microscope, spherical particles were perceived, though few in number, when duly diluted. The presence of an alkali I could in no instance perceive, by means of the usual tests, *namely*, turmeric paper, litmus paper slightly reddened by vinegar, and cloth stained with violet juice; nor was an acid denoted by means of litmus paper, except when I had

I had reason to believe it was derived from various acid substances taken with the food, or drink, adhering to the inside of the mouth and fauces.

Mucilagelike matter.

2. *The mucilagelike expectorated matter*, according to my observation, occurs much less frequently than the other sorts. It appears suddenly in great abundance in certain bronchial catarrhs. I have seen it to the amount of two or three pints in twenty-four hours. It is also secreted, but less copiously in paroxysms of spasmodic asthma, and of the hooping cough; and but rarely in pneumonic, or pleuritic inflammations, and in some chronical organic diseases of the heart and lungs.

Described.

This matter is a transparent uniform fluid of the consistence of white of egg; or of a mucilage compounded of about one part of Arabic gum, and four or five parts of water. It is colourless—has a fleshy smell—has a brackish taste. After standing eight or ten hours, a deposit takes place of fibrous, leaflike, or curdy masses, some of which are seen suspended in the clear fluid. In some cases nodules of opaque thick ropy matter, at certain times, accompany this mucilagelike matter. Under the simple magnifier I perceived irregular figured masses partly in motion and partly suspended, With the microscope, globules were seen; but larger considerably than those of the blood, and much less numerous. With the usual tests there were no indications of alkali, or of acid, provided the matter was unmixed with other things. It usually floated, or was suspended in water, when first expectorated; but on standing in the water it fell to the bottom, evidently owing to the disengagement of air bubbles.

By standing exposed to the air in warm weather, it sooner grew fœtid than pus of abscesses; without becoming opaque. Neither could I render it opaque or thicker, by exposure to a stream of oxigen gas for an hour; or by exposure of it in a jar of this gas for a month.

3. *The opaque ropy matter* above-mentioned.

Opaque ropy matter.

1st. It is secreted most copiously in that very common, and extensively epidemial disease of our climate, the *winter-cough*, occasioned by tubercles, to the amount of half a pint or a pint in twenty-four hours; especially during the winter season for several successive years, and sometimes during the whole of a long life, after the age of forty or fifty years. 2dly. It is often the expectorated matter of the pulmonary consumption of young persons, also occasioned by tubercles, but frequently mistaken for the pus of abscesses or vomicæ. 3dly. It appears, oftentimes, in pneumonic or bronchial inflammation with fever, seemingly being a beneficial discharge; as well as in some instances at the close of a fever without concomitant inflammation of the lungs. 4thly. A severe paroxysm of spasmodic asthma is often terminated in the excretion of this kind of matter. 5thly. A secreted substance of this sort is sometimes expectorated in various chronical organic diseases of the lungs, the heart, aorta, and parts contiguous to the lungs, which occasion difficult transmission of blood through them.

Described.

In all these instances the matter by expectoration is of the consistence of thick cream, or of thin toasted cheese; so tough as to hang in the form of a rope, four or five inches in length, on pouring it from one vessel into another. Its aggregation is such, that it is readily detached in large masses from the vitreous surface of vessels. It is not unusual for small black, or reddish spots, and streaks, to appear on the surface of this sort of expectorated substance. A pretty large bulk of it is seldom throughout uniform; but it is frothy, and exhibits opaque masses of various hues with transparent matter interposed. The *colour* is yellowish, straw-coloured, and white, or gray: it also, though seldom, is greenish and bluish. The *taste*, asserted by patients, is, in their own terms, various, *namely*, saltish, nasty, faintish, sweetish, luscious, or like that of a sweet oyster,—a sharp or sour taste is the most rare. The only *smell* which I have perceived is that of flesh, but very frequently there is none. When any offensive or pungent smell was perceived, immediately after expectoration, I have always found that it was owing either to the foulness of the vessel in which it was received; or it was from extraneous matters in the mouth, and from decayed teeth.

irculating pherical globules.

This opaque viscid substance, being duly diluted with distilled water, was examined with microscopes of common, as well as of very great powers: by means of any of them crowds of spherical particles were seen passing to and fro, in currents, not unlike those of the blood; except that they were larger. These globules I could not destroy, or alter in form, by trituration; or by long boiling in water; or by exsiccation, and again dissolving in water; or even by coagulation with mineral and vegetable acids, with alcohol, with sulphuric ether, or with tannin, and alum; or by mixture with caustic alkalis in a proportion which leaves the liquor turbid; or for some time after the putrefactive process had appeared. But these globules disappear with such a proportion of sulphuric acid as detaches charcoal; or of nitric acid, and of liquid potash, as produces a clear solution: also by charring by fire. It is perhaps superfluous to remark, that these atomic globules are quite different from the air bubbles usually entangled in this kind of matter, as perceived by the microscope; the latter differ much from the former, in being of far greater magnitude—in being less numerous—in being transparent, and disappearing on agitation, or heating the matter, or even by mere standing.

For the most part this expectorated substance swims on water; but by agitation or stirring to disengage air bubbles, or by merely standing, it sinks. Some of the lumps suddenly hawked up immediately fall to the bottom of a vessel of water. No signs of either acid, or alkali, appeared on the trials of this matter with well known reagents, provided it was free from extraneous matter; but it was apt to betray acidity from things taken with the food or drink.

Puriform matter.

4. *Puriform matter.* I have seen this matter expectorated in several diseases in the quantity of two or three ounces to half a pint in twenty-four hours, on some rare occasions, without any breach of surface. I believe it would be considered by every one to be *pus*, having the properties commonly admitted to be those of this substance. It will however, perhaps, only be just to call it *puriform*, for the present, as it appears to me probable, that I shall hereafter be able to show, that it possesses properties not belonging to pus of abscesses, although in the obvious, or sensible properties, it is similar to such pus. Accordingly this expectorated matter is not only opaque, white, or yellowish, and as thick as the richest cream, but it also has not more tenacity

city than cream. It is not apt to entangle air, and therefore it immediately mingles with water, rendering it milky; and presently subsides to the bottom, leaving the water clear, or at least whey-coloured. It appears to the naked eye uniform in its texture; and nearly so under the simple lens: but under the microscope thousands of globules similar to those of the blood are seen, which are indestructible as those above related belonging to another kind of expectorated matter.

ot from ul-
r.ion.

The substance, of which I am now speaking, is most frequently excreted in the latter stages of pulmonary phthisis, for many weeks successively. It is taken for granted, that this matter is from a breach of surface or ulceration; but on examination after death, such a state was not found, in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. This puriform matter is occasionally expectorated in certain other diseases. The last summer my colleague, Dr. Nevinson, furnished me with several ounces of this sort of substance, but of a greenish hue, and of the consistence of thin cream; which was expectorated by a woman in the third week from the attack of the measles. In a few days she died. On the examination of the lungs very carefully, by the excellent house surgeon of St. George's hospital, Mr. Dawes, no ulceration could be discovered in the trachea or in the bronchial tubes; nor were any tubercles, or abscesses found in the lungs. The patient, according to my information, had expectorated more than a pint of this fluid every twenty-four hours for a week before death. In another hospital case, a man laboured under a cough with spitting of matter, which all who saw it called pus, and as usual it was considered to arise from an ulceration, or suppurated tubercles; but, on examination after death, the disease was ascertained to be condensation of the lungs to the consistence of liver, with water in the cavities of the chest, and nothing more.

paque viscid
atter.

5. *Opaque viscid matter of the third, and perhaps fourth sort,* above distinguished, appearing in nodules, and irregular figured masses, *mixed with transparent slimy matter of the second sort.*

It is not unusual to see the mixture of these two different kinds, from severe fits of coughing in that constant epidemy of the British islands, the winter chronical pneumonia.

Different parts of the bronchial membrane being in different states may account for the secretion of the two different matters. This seems more probable, than that these different matters should be secreted from the same part; although it is true, that the same part does secrete at one period transparent thin slime, and at another an opaque thick matter. The former is occasioned by great irritation of the membrane, and the latter is the effect of a more gradual secretion with much less irritation.

For the sake of brevity I avoid a farther description. The practical application of these observations, however important, would or be suitable in this place.

The sixth and seventh kinds of expectorated substances being secreted after a quite different manner, and being very different in their nature from the preceding five kinds, I shall not give an account of them in this paper.

SECT. II. *Agency chiefly of Caloric.*

Chemical analysis.
Action of heat.

1. No effect of importance is produced by this agent, until the temperature of the expectorated matters is raised to about 150° of Fahrenheit's thermometer: then the state of aggregation is evidently altered, the viscidity of each of them being diminished. At about 155°, coagulation begins to be quite evident in the first, third, fourth, and fifth kinds of matter—that is, curdy masses of various magnitudes appear in a milklike or whitish liquid. On elevating the temperature to 160° or 170°, a large proportion of curd is formed; but the proportion of the curdy matter to the liquid is very different in different specimens. The viscid texture, or tenacity of the expectorated matters, is by this treatment destroyed. The milky liquid decanted, after standing ten or twelve hours, affords, on evaporation to dryness, about three or four grains of residue from each 100 grains.

This liquid passes very slowly through the paper filter. The filtrated liquor affords scarcely more than one per cent on evaporation to dryness. By repeatedly boiling in successive portions of water, the whole, as far as I could judge, of a given quantity of the curd might be diffused to form a whitish liquid; which on evaporation to dryness appeared to afford a residue of the same kind (except in containing a smaller proportion of saline substances), as the milky liquid, which was separated from the curd on the coagulation of the expectorated matter.

The second kind, called *mucilagelike transparent matter*, does not afford curdy masses at the temperatures abovementioned; but its viscid texture is destroyed, and it becomes a wheylike, or ſomewhat milky liquid; and, on examination with a magnifying glass, it appears full of curdy particles. After this agency of caloric, the expectorated matter is much less prone to putrefaction.

Distilled fluid.

2. Distillation of the expectorated matters to dryness afforded a particularly limpid water, which had a peculiar smell, but no impregnation with ammonia; or with any other substance which could be detected, except a little carbonic acid.

Residuum.

The residuary matter, in a brittle state of dryness, afforded by evaporation, varied between two and a half and ten per cent of the expectorated substances. The second kind yielded from one thirty-fifth to one forty-fifth of its weight of brittle residue. The first kind afforded from one twentieth to one twenty-fifth of residue. The third kind afforded very different proportions of solid residue, according to its consistence, viz. from one tenth to one eighteenth of its weight. The fourth kind gave from one twelfth to one fourteenth of brittle matter. The fifth kind yielded very different proportions of residue, according to the very different proportions of transparent and opaque matter, of which it consisted—it varied between one eighteenth and one thirtieth.

Attracted moisture from the atmosphere.

3. All these exsiccated substances on exposure to air grew more or less moist, or at least were no longer brittle; but became somewhat soft, and proportionately to the state of moisture, were augmented in weight. The thinner the expectorated matters, the moister, and the greater increase of weight they generally experienced. But parcels of the same consistence from different patients sometimes differed

much in degree of moisture on exposure to the air. I have found some parcels of the second and fifth sorts of expectorated substances grow quite moist, and receive an increase in weight of three per cent. If the residues were kept in close vessels, they remained in a brittle state. Larger parcels of exsiccated matter become more moist than smaller ones of the same kind in the same circumstances.

Milky and curdy liquids, and curdy matter exsiccated.

4. The milky and curdy liquids, which separated from the curdy masses (1) being poured off; and also the curdy masses being by pressure rendered dry; the liquids were evaporated to dryness, but became moist on exposure to the air. The curdy masses were by evaporation rendered brittle, and remained so in the air. The residues of the evaporated liquids were said to taste extremely salt, and the exsiccated curdy matter was tasteless.

Milky liquids.

5. The milky liquids (4), concentrated by evaporation, did not indicate any disengaged acid, or alkali, to the usual reagents.—By triturating these liquids with lime, a little ammonia was discharged—by trituration with concentrated sulphuric acid, the muriatic acid was disengaged—with phosphoric acid, and also with tartaric acid on trituration and heating, a pungent smell was perceived, somewhat like that of the acetous acid. On burning to a brown ash the saline residue afforded by evaporation of these liquids, the predominating taste of it was that of muriate of soda. This ash readily melted,—being moistened, it turned turmeric paper to a reddish brown colour, and changed turnsole paper, reddened by acetous acid, to a deep blue—on exposure to the air, it partially deliquesced—the dissolution, by boiling in distilled water, afforded supertartrate of potash on the addition of the tartaric acid; and a red precipitate was occasioned by nitro-mutiate of platina*. This incinerated and fused saline residue by other trials was proved to contain phosphoric acid and lime; with traces of sulphuric acid, magnesia, iron, and perhaps silica; but the chief ingredients were muriate of soda and potash.

Curdy matter.

6. The curdy matter after expression (4) afforded a much smaller proportion of brown ash than the fusible saline

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* The knowledge of this reagent, I believe, the chemical world owes to Dr. Wollaston.

Opaque ropy matter.

like residue (5). It required an intense fire for fusion in a platina crucible. The fused mass did not deliquesce, but it grew somewhat moist on exposure to the air. It contained a much smaller proportion of potash than the former fused matter (5); also much less of muriate of soda, but a far larger proportion of lime and phosphoric acid, with traces of sulphuric acid, magnesia, oxide of iron, and perhaps silica.

7. (*a*) 15400 grains of the third sort of expectorated matter on exsiccation afforded 960 grains, that is, one sixteenth of brittle substance, or about six per cent, and of course this kind of matter contained about ninety-four per cent of water (sect. II. 2). This dried matter was reduced to a charred state by exposing it to fire in a Wedgwood white crucible. In this process it inflamed, emitted the usual smell of burning animal matter, especially of bone, and swelled prodigiously; at the same time a black oil was compounded rendering the mass soft during the inflammation. I could not distinguish the smell of sulphur, but there was in one part of the burning, a smell, to my sense, of phosphorus.

(*b*). This charred matter was kept in a state of ignition in a platina crucible, till it no longer remained in a powdery form, but was reduced to a comparatively small bulk of a substance of the consistence of paste in an intense fire. By continuing the fire, the charge at length was fused; and after being kept in a state of fusion to be quite fluid for ten minutes, the fire being withdrawn, a white, brittle, apparently saline matter, like melted common salt, was easily detached from the platina crucible, which in some places had received a red tinge.

(*c*). The melted matter (*b*) weighed fifty-nine grains: of course, this saline residue amounted to $\frac{1}{261}$ of the expectorated matter, and to one sixteenth of this expectorated matter exsiccated. It tasted only of muriate of soda—it had no smell—it effervesced with acids—it betrayed the presence of alkali to the tests above-mentioned—after a few days exposure to the air, it partially deliquesced—it precipitated supertartrate of potash with tartaric acid, and emitted no ammonia with lime, or sulphur with muriatic acid, discoverable by the most delicate tests.

(*d*). The

Opaque ropy matter.

(*d*). The fused matter (*c*) was boiled with three times its weight of distilled water, in which about one half appeared to dissolve. The clear liquid, decanted from the sediment and evaporated, yielded crystals of muriate of soda with a much smaller quantity of spicula, or needle-shaped crystals; and saline matter which appeared under a lens not definitely crystallized. A second boiling of the sediment, with twice its quantity of water, afforded almost entirely muriate of soda. A third boiling gave a few crystals of this salt only, as appeared under the magnifier. A fourth boiling, in an equal weight of water, afforded no saline matter.

(*e*). The saline matters (*d*) amounted to forty-five grains when evaporated to dryness. I collected by means of a toothpick, from amongst the cubical crystals, as much as I could of the spicula and uncrystallized saline matter. These parts effervesced and precipitated supertartrate of potash with tartaric acid, and certainly afforded no soda-tartrate of potash—they also afforded a precipitate with nitro-muriate of platina—being saturated with acetous acid there was still a slight precipitation with muriate of baryt; for without acetous acid, there was a most copious precipitation with this reagent, but the greater part of the precipitate was dissolved by acetous acid, added so as not to supersaturate it.—Oxalate of ammonia did not occasion a precipitation,—with nitrate of silver an abundant one took place—lime water produced only slight turbidity. The muriate of soda amounted, in this saline mass of forty-five grains, to thirty-five grains, or nearly to one grain in 450 of expectorated matter; the rest was subcarbonate of potash amounting to one grain, in about 1540 grains of expectorated matter, with which was mixed a minute proportion, probably, of sulphate and of phosphate of potash.

(*f*). The undissolved matter (*d*) boiled with muriatic acid gave a turbid liquid, but on standing, nearly the whole appeared to have been dissolved: a small proportion of sediment only took place in a transparent liquid, which was boiled till it no longer parted with muriatic acid.—This dissolution being exsiccated grew liquid on exposure to air; and oxalate of ammonia, gradually added, produced, as I decidedly ascertained, the precipitate of oxalate of lime.

(*g*). The

Opaque ropy matter.

(*g*). The filtered residuary liquid (*f*) with muriate of baryt gave immediately a copious precipitation—with lime water there was milkiness produced, and subsequently a white precipitation which did not disappear on adding a small proportion of acetous acid—prussiate of potash occasioned a greenish blue colour without precipitation—succinate of ammonia produced a milky liquid—no effect was observed from tartaric acid—There being a precipitation with caustic or pure ammonia, as well as with potash, and with the carbonates of the alkalis, it was supposed magnesia was present: and the dissolution of this precipitate in muriatic acid, and in acetous acid, gave no precipitate with oxalic acid. Some of the muriatic dissolution, previously to precipitation with oxalate of ammonia (*f*), being evaporated to dryness, the residue was ignited; but if magnesia was present, as well as lime, it was in too small quantity to be distinguishable from the lime, by composing sulphate. The precipitate now under examination was certainly not mere magnesia, for it melted into an opaque globule under the blowpipe; it was not phosphate of lime, for with sulphuric acid a somewhat bitter and sour substance was compounded, which afforded a precipitate with ammonia, but none with oxalate of ammonia. It was a phosphate not only on account of its fusibility, but because a curdy appearance was occasioned by the mixture just mentioned with sulphuric acid, on adding it to lime water. Neither was it soluble, like phosphate of lime, in phosphoric acid. The quantity of this precipitate was too minute for decisive experiments, but from those related, it seems probable that it was phosphate of magnesia, which was dissolved, as will appear presently, in phosphoric acid, and precipitated by ammonia.

(*h*). The residuary liquid (*g*), after the precipitation by oxalate of ammonia, being evaporated to dryness, was easily ascertained to be phosphate of ammonia, with indications of a minute proportion of sulphate.

(*i*). It remains only to notice the indissoluble matter in muriatic acid (*f*). I found it to grow soft, and the parts to cohere under the blowpipe, and with a little potash it readily melted into an opaque globule.

8. To obtain a more satisfactory proof of the presence of sulphur, forty grains of charred expectorated matter were kept in a state of ignition in a platina crucible, with another inverted over it to exclude completely the escape of gas, for two hours. After cooling, the smell of sulphuretted hidrogen gas was very evident, on the addition of diluted muriatic acid, and even of water only. Silver was tarnished, and paper wetted with liquid acetite of lead was blackened by this gas. In some of the experiments, while the charcoal was burning off from the charred expectorated matter, I perceived the smell of sulphur, and perhaps of phosphorus.

(To be concluded in our next.)

XII.

On the Nonabsorption of Oxigen in Respiration, in Reply to Mr. ACTON, *by* J. F.

To Mr. NICHOLSON.

SIR,

Defence of Mr. Ellis against the charge of self contradiction.

MY former remarks on Mr. Acton's communication were designed to rescue Mr. Ellis from the imputation of "perverting the experiments" of Mr. Bichat.—My present aim is to defend the same writer from the charge of contradicting himself. "If," says Mr. Acton, "two experiments of an opposite nature, mentioned doubtless to prove the truth of the general proposition, can be considered a contradiction, then has Mr. Ellis contradicted himself." To this I reply, that a general proposition may be supported by dissimilar proofs, which require experiments of an opposite nature, without involving any contradiction: and such I take to be the case in the experiments referred to by Mr. Acton. To prove, that air does not naturally enter into the blood, Mr. Ellis quotes an experiment of Hales, who injected air into the lungs of an animal, but it did not enter the vessels. As a farther argument against this opinion, Mr. Ellis also states, that, if air be made to enter the blood, it quickly destroys life; and he refers for proof of this to an experiment of Mr. Bichat, who not only, like Dr. Hales, injected air into the lungs, but *confined* it there, by which means it did enter into the blood vessels, and speedily proved fatal. Where now is the contradiction in all this? Two different positions, tending doubtless to the support of one general proposition, were, by these experiments to be established; and, if the experiments be deemed correct, they go directly to fulfil their destined purpose. In his zeal against the general proposition, Mr. Acton seems to have overlooked the varying nature of the proofs adduced in its support, and he thus sees contradiction in opposite experiments directed to the establishment of dissimilar truths.

Messrs. Allen and Pepys found the oxigen lost in respiration converted into carbonic acid.

In my postscript I observed, that the late experiments of Messrs. Allen and Pepys seemed to support Mr. Ellis's opinion, that "all the oxigen gas lost in respiration was to be found in the carbonic acid produced." As Mr. Acton pronounces this an "inaccurate and unfair statement," I must beg leave, in justice to myself and to those gentlemen, to deliver a short abstract of their experiments nearly in their own words. In the analysis of the respired air of thirteen experiments, in each of which nearly from 3 to 4000 cubic inches of air were *once* passed through the lungs, and in one instance little less than 10000, where "the breathing was nearly natural, the operator scarcely fatigued, and his pulse at the end of the experiment (which lasted from 5 to 24 minutes) not raised more than about one beat in a minute," these gentlemen found the quantity of oxigen gas and carbonic acid, taken together, to amount always to 21 parts in the hundred, which exactly corresponded to the proportion of oxigen gas, previously ascertained to exist in the respired air: wherefore they concluded, "that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxigen consumed." This conclusion is just what it should be, a simple expression of the fact, and nothing more; and it goes entirely to support the opinion of Mr. Ellis, to which I before alluded.

In some cases however oxi-

But in two other experiments, which lasted only two or three minutes, where not more than 300 cubic inches of air were employed, which were passed *eight* or *ten times* through the lungs, "until respiration became extremely laborious, with a great sense of oppression and suffocation in the chest, indistinct vision, buzz in the ears, and at last perfect insensibility," the oxigen and carbonic acid expelled amounted together only to 14 or 16 parts in the hundred, instead of 21, as in all the former experiments; and therefore when, as in these experiments, "respiration is attended with distressing circumstances, there is reason," say these gentlemen, "to conclude, that a portion of oxigen is *absorbed*." This conclusion, however, unlike the former, evidently involves matter of *opinion*, as well as matter of *fact*. To the fact, that a portion of oxigen was lost, I readily assent; but to the opinion, that this oxigen was absorbed, I must beg leave to demur. The two series of experiments differ not less in their chemical results, than in their effects on the animal system; and although these gentlemen, at the commencement of their excellent memoir, seem to have been fully aware, "that the deficiency in the respired air principally arises from the difficulty in bringing the lungs precisely to the same state after as before the experiment," yet they appear to have passed by this consideration in forming the above conclusion; and have thus been led to consider the mere disappearance of a portion of the inspired air as a proof of its *absorption by the blood*, when, in reality, it proves nothing more than its *retention in the lungs*.

gen disappeared.

This supposed to have been retained in the lungs.

I have thus endeavoured to separate fact from opinion in these experiments; and, if I mistake not, the fact is with me and Mr. Ellis—the opinion with Mr. Acton.

J. F.

REMARKS.

Controversy requires limitation.

As it is but equitable, that every writer should be allowed to defend the opinions of himself or his friend, as far as may be done without indulging endless or unnecessary controversy, the preceding paper has been admitted: though I apprehend nothing remains now to be said on either side, unless fresh facts can be adduced, that may tend to decide the point at issue.

The oxigen disappearing in some cases of respiration not simply retained in the lungs.

I would just observe however, that J. F.'s mode of accounting for the deficiency of the proportion of oxigen in the last two experiments of Messrs. Allen and Pepys quoted by him appears to be founded on an erroneous idea. These gentlemen did not ascertain the absolute quantity of oxigen in the air after respiration, they merely determined the proportions of oxigen and carbonic acid to nitrogen in a given quantity; consequently, whether the air left in the lungs at the end of the experiment were more or less, this air would have nearly the same proportions of oxigen, carbonic acid, and nitrogen, as that in the gasometer; so that no surplus in the lungs would account for the deficient proportion of oxigen.

The mode of conducting the experiment shows this.

There is another remark may be made on this subject. The operator, in the experiments of Messrs. Allen and Pepys, draws the air into his lungs from one vessel through one tube, and then breathes it out through another tube into another vessel; and this he continues to repeat a considerable number of times, all communication between his lungs and the external air being completely cut off both in expiration and inspiration. Consequently all the loss of quantity in the result, that can be ascribed to the lungs being less completely emptied at one time than another, must be the surplus, that the lungs retain after the last expiration above what they retained after the expiration previous to commencing the experiment. In the third experiment of Messrs. Allen and Pepys [see Journal, vol. XXII, p. 183] this loss is only four cubic inches, which might be ascribed to such a cause: but in several others it is upwards of thirty, and in the eighth the loss is sixty-two cubic inches, which is surely much too great to be accounted for in this way.

Apparent miscalculation of Messrs. A.&P.

I am inclined to suspect, that Messrs. Allen and Pepys themselves, when they ascribe the deficiency "principally to the impossibility of bringing the lungs to the same state after forcible expiration" [ib. p. 187], divided the quantity thus disappearing among the total number of expirations made; for this remark is introduced by the following words: "The smallness of the deficiency

[18 cub. inches], notwithstanding the experiment occupied 24½ minutes, &c." But the duration of the experiment could have no effect on the result as far as it was owing to the cause in question, since this is simply the difference between the air retained in the lungs previous to the experiment and after its conclusion, whether the experiment consisted of a single inspiration and expiration, or of a thousand. C.

XIII.

Observations on the Pickle of Violets, considered as a Reagent; and on the Advantage of Salting Vegetables, from which Distilled Waters are intended to be made: by Mr. DESCROIZILLES, *Sen.*[*]

Sirup of violets apt to spoil.

IN chemical analyses, when we wish to detect the presence of uncombined acids or alkalis, or of alkaline subcarbonates, the most usual test is sirup of violets. But this has some inconveniencies, as it is very apt to spoil. Accordingly it occurred to me to try what I call pickle of violets, and I found it answer extremely well. The following is my mode of making it.

A saline infusion preferable

Mode of preparing it.

On the petals of the violet slightly pressed into a small pewter measure, pour double their weight of boiling water, and stir them together. Cover over the measure, and expose it for a few hours to a heat somewhat greater than that of a waterbath: after which let the liquor be strongly pressed out through a very clean linen cloth. Weigh the infusion accurately, and add to it one third of its weight of common salt, stirring it till this is dissolved. Very fine white salt should be chosen, as this contains little or no earthy muriate, which would be detrimental to the colour. In a small phial corked this will keep without alteration, though exposed to various degrees of temperature, and even to the rays of the sun. As a reagent it is far preferablé to the best sirup of violets*.

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[*] Annales de Chimie, vol. LXVII, p. 80.

Other blue flowers would answer.

It may be presumed, that several other blue flowers, as those of the iris, larkspur, &c., would afford a pickle of sufficient sensibility. The latter I have tried with success.

Method of using it.

To use this blue pickle, dip into the phial the end of a little stick, or of a match with the brimstoned part broken off; and with this end touch a clean earthen plate in various places. On one plate you may make thirty such spots for trial, each of which will not require above a quarter of a drop: so that a few drachms of this pickle will last a twelvemonth, though you have frequent occasion for its use.

Vegetables might be preserved for distillation by salting.

It appears that the utility of common salt in preserving vegetables required to be brought from a distance, for the use either of the apothecary or perfumer, has not been in general sufficiently understood. Hilary Marin Rouelle, whose pupil I was, perfumed his laboratory during a whole course of chemical lectures in the winter of 1775 by distilling roses he had salted in the month of june. The rosewater he obtained, being mixed with sugar and alcohol, formed a delicious cordial. I have kept a jar full of salted roseleaves in my laboratory these three years, and their perfume has lost nothing of its strength or sweetness. They may be salted in the following manner.

Process of salting.

Take a kilogramme and half [4lbs. troy] of roseleaves, and bray them for two or three minutes with one third their weight of common salt. The flowers, bruised by friction with the grains of the salt, will presently give out their juice, and produce a kind of paste of little bulk. This you will put into an earthen vessel, or small cask, and proceed in the same manner, till you have filled it. All your roseleaves being thus salted in due proportion, you will stop the vessel close, and keep it in a cool place till wanted.

Distillation.

Whenever you think proper, you may proceed to distil this fragrant paste at your leisure, putting it into a common still, and diluting it with about double its weight of pure water. Thus you will neither be hurried by the season, nor inconvenienced by distance; for during the winter you may distil at Paris herbs salted long before in places the most remote from the capital.

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* In 100 parts of sirup there are 66 of sugar, which often contains some lime. In 100 parts of the pickle there are but 25 of salt.

Applicable to herbs, the water of which will not keep.

According to some, distilled waters thus obtained are more fragrant, and at the same time yield more essential oil. Lastly I would add, that this mode of salting may be applied to some very useful purposes. For instance, if it be true, that the waters of some herbs will not keep the year round, though distilled with the utmoſt care; these herbs, if properly salted, will keep; so that they may be distilled when wanted, and their water employed while in full possession of its medical virtues.

XIV.

On the Existence of Oxalic Acid in the Leaves and Stalks of the Rheum palmatum, or true Rhubarb; by Mr. B. L*.

Oxalate of lime in rhubarb root.

It is well known, that Scheele observed the existence of oxalate of lime in the root of rhubarb; but I am not acquainted with any person's having examined the juice, ſtalks, and leaves, of the rheum palmatum. Mr. Vogel and myself, surprised at the quantity of free acid in this plant, endeavoured to ascertain its nature.

Juice of the leaves & stalks.

We first pounded the leaves and stalks in a wooden mortar, expressed the juice, and filtered it.

Its properties.

It was clear, with a slight yellowish tinge. Its smell was weak, and somewhat like that of melilot. Its taste was evidently acid, and it reddened the paper and infusion of litmus very powerfully.

Examined with different reagents.

The pure alkalis and their carbonates changed its colour to a deep brown, without occasioning any precipitate.

The oxalate of ammonia produced no change in it.

With limewater a whitish precipitate was formed, insoluble in water, but soluble in acids.

The muriate of lime likewise occasions a very copious white precipitate, insoluble in water, but soluble in nitric acid.

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* Annales de Chimie, vol. LXVII, p. 91.

The precipitate produced in it by acetate of lead is of a yellowish white.

That by nitrate of mercury is white, and in a great measure soluble in nitric acid.

Nitrate of silver produces the same phenomena.

Muriate of tin gives a yellow precipitate with it.

Contains a free acid analogous to the oxalic.

These preliminary trials show: 1, that a free acid exists in the plant: 2, that this acid is analogous to the oxalic, which is demonstrated by the precipitates obtained with limewater and muriate of lime: 3, that the oxides of lead and tin seize on a colouring extractive matter: 4, that this juice holds no calcareous salt in solution, since no precipitate is produced in it either by the pure alkalis, or their carbonates, or oxalate of ammonia.

The juice distilled.

No acid came over.

Desirous of ascertaining whether any volatile acid existed in this liquor, we distilled the filtered juice in a retort on a sand-heat. What came over was a perfectly clear water with a slight aromatic smell, not acid, and yielding but a slight precipitate with acetate of lead, which precipitate was soluble in nitric acid. We perceived no effect from limewater, baryteswater, or muriate of tin.

Residuum contained superoxalate of potash,

What remained in the retort was evaporated slowly till it had acquired nearly the consistence of a sirup. At the expiration of four and twenty hours a pretty large quantity of small crystals were found at the bottom of the saucer, which, when separated and washed, exhibited all the characteristics of acidulous oxalate of potash.

and some acid, probably the acetous.

The supernatant fluid was highcoloured, and yielded no more crystals by evaporation. It contained scarcely any more oxalic acid, for it formed no sensible precipitate either with limewater or muriate of lime, yet it was very sour.

Though the distillation of the juice, as mentioned above, yielded no acetic acid, we are inclined to believe, that this acid exists combined, or retained by the colouring extractive matter, as is the case with the juices of a great number of plants.

The residuum dried, and treated with alcohol.

The evaporated mass reduced to the state of powder we digested in alcohol at 40. The alcohol acquired a light yellow tinge, and was acid; but the acid was not the oxalic.

Indeed it is well known, that the acidulous oxalate of potash is scarcely soluble in this menstruum.

Potash found in it.

The residuum that was insoluble in alcohol contained acidulous oxalate of potash. Being calcined and incinerated in a platina crucible, a very alkaline, white, melted mass was obtained. Its concentrated solution changed the acid sulphate of alumine into alum. This substance therefore was nothing but nearly pure potash, a little carbonated, and containing a very small quantity of sulphate and muriate of potash.

Results.

From this examination of the juice of the stalks and leaves of the rheum palmatum, or true rhubarb, it follows, that it contains, 1, a pretty large quantity of acidulous oxalate of potash: 2, an uncrystallizable acid combined with the colouring extractive matter, which is analogous to the acetic acid, and in this state exhibits some of the properties ascribed to the acid termed malic: 3, and lastly, that the presence of this acid confirms in some measure the experiments of the celebrated Scheele. We need not be at all surprised to find oxalate of lime in the root of rhubarb, since an acidulous oxalate of potash is obtained from its leaves.

Various roots contain oxalate of lime.

Rhubarb is not the only root, that contains oxalate of lime, for Scheele found it in several others; but as this salt exists in them in very small quantity, he employed a particular process to separate it, which we shall insert here, to save trouble to those who would institute similar inquiries.

Method of detecting it.

Cut and bruise the roots, and pour on them muriatic acid diluted with a great deal of water. Leave them to digest a few hours; then filter the solution, and saturate it with ammonia. If the roots contain any oxalate of lime, this salt will be dissolved by the acid, and precipitated by the alkali.

Roots in which it has been found,

The following substances afforded this chemist more or less oxalate of lime. 1, the roots of alkanet, parsley, carline thistle, turmeric, white dittany, fennel, red gentian, swallowwort, patience dock or rhubarb, liquorice, mandrake, restharrow, Florentine orris, soapwort, squill, tormentil, valerian, zedoary, and ginger; and 2, the barks of cascarilla, cinnamon, elder, and simarouba.

and barks.

XV.

XV.

Analysis of the Aplome: by Mr. LAUGIER[*].

Found on the banks of the Lena.

MR. Haüy has given the name of aplome to a stony substance found in Siberia on the banks of the Lena. We are indebted for a knowledge of the place whence it is brought to Mr. Weiss, who has parted with his collection to government. The aplome has some resemblance to the garnet and the idocrase, or brown hyacinth of volcanoes. It differs from the former in its specific gravity, which is 3·444, though it crystallizes like the dodecaedral garnet; and from the latter in its primitive form. The figure of the crystals of the aplome seems to indicate, that they are the result of a decrement by a single row on each edge of a cube. "This decrement," says Mr. Haüy, "is so simple and elementary, that I had selected it for the first in explaining my theory of the structure of crystals." This induced him to give it the name of *aplome*, or simplicity.

Has some resemblance to garnet and idocrase.

Loss by calcination.

Treated with potash and muriatic acid.

The aplome loses by calcination two per cent. A hundred parts of aplome reduced to fine powder were treated with four hundred of caustic potash. A red heat kept up for half an hour produced only a pasty fusion. The mass when cold was of a bottle green colour. The muriatic acid completely dissolved it. This solution, treated in the usual way, gave for every hundred parts of alumine

Its component parts.

Silex	40
Alumine	20
Lime	14·5
Oxide of iron	14·5
Oxide of manganese	2
A mixture of silex and iron	2
Loss by calcination	2
	95
Loss	5
	100.

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[*] Journal de Physique, vol. LXVII, p. 392. Abridged from the Annales de Musée.

This analysis shows, that the aplome cannot be considered as any of the minerals yet known.

A new species.

Under these circumstances it seems to me, when the results of analysis are not sufficiently decisive to settle the point, it is indispensably necessary to combine with its results the geometrical and physical properties of the mineral; and if we take this course here, which is unquestionably admissible, we shall be led to consider the aplome as forming a distinct species.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineralogy of Ben Ledi.

AT the meeting of this Society on Saturday the 13th of january last, the Rev. Dr. Macknight read a Mineralogical acccount of Ben Ledi, and the environs of Loch Katterin. The description of the rocks in that district (which consist of mica-slate, and clay-slate, with an overlying conglomerate, formed at a lower level from the debris of primitive mountains) tended, in the author's opinion, to illustrate one branch of the Wernerian doctrine, respecting the order of formations in the mineral kingdom. It also appeared, in confirmation of another principle in the Geognosy, that the direction from SW to NE of the strata composing the Highland mountains, corresponds to what has been observed *in general* relative to the bearings of the primitive strata in the crust of the Earth. Such a uniformity of direction, it would seem, could have resulted only from the action of powers in nature that are slow and regular in their operation; and must be referred to some original law, which, later discoveries render it probable, will be found to depend on the constitution of the terraqueous globe with regard to magnetism and electricity.

Meteorology of Greenland.

At the same meeting, the Secretary laid before the Society a communication from Mr. Scoresby junior of Whitby, comprising a meteorological journal of three Greenland voyages; with remarks on the effects of the weather on the barometer in Greenland; and on the different crystallizations of snow to be observed in high latitudes.

Lectures on electricity.

Mr. Singer, of the scientific institution, Princes street, Cavendish square, intends to commence a course of fourteen lectures on electricity, introductory to his course on electrochemical science, on the 6th of march.

ERRATUM.

Vol. XXIV, p. 374, line 26. *For* nitrogen *read* hidrogen.

METEOROLOGICAL JOURNAL,

For FEBRUARY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JAN. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Day.	Night.
28	31°	31°5	32°5	30°	30·23	Cloudy	Cloudy
29	31	33·5	34	31·5	30·27	Ditto	Ditto
30	33	34·5	34	30	30·44	Ditto	Ditto
31	33·5	44·5	45	42·5	30·35	Rain	Rain
FEB.							
1	47	48	49	45	30·17	Rain	Cloudy
2	46	46	47	42	30·09	Ditto	Rain
3	44	45·5	46	41	29·80	Ditto	Cloudy*
4	42·5	38	43	33	29·90	Ditto	Fair
5	36	41	44	33	30·08	Cloudy	Ditto
6	40·5	45	46	43	30·04	Ditto	Ditto
7	46·5	46	49	43	29·95	Ditto	Cloudy
8	45·5	47	48	41	29·97	Rain	Rain
9	44	47	47·5	46	29·80	Cloudy	Cloudy
10	48	46	49·5	40	29·78	Rain	Foggy
11	42	41	43·5	38	29·87	Ditto	Rain
12	41	40	42	36·5	29·35	Ditto	Cloudy
13	38	39	42·5	34	29·05	Fair	Fair
14	36·5	37·5	40	33·5	29·34	Foggy	Foggy
15	36	35	39	27	29·70	Cloudy	Fair
16	30	36·5	37.5	27·5	30·05	Fair	Cloudy†
17	30	32·5	35·5	25	30·10	Ditto	Fair
18	29	34·5	34·5	27·5	30·17	Snow	Rain
19	30	31·5	35	26	30·00	Fair	Fair
20	30	27	32·5	20	30·20	Snow	Ditto
21	22·5	30·5	31·5	25	30·43	Fair	Ditto
22	30	30·5	32·5	32.5	30·28	Ditto	Ditto
23	36	41	43	38	29·05	Rain	Cloudy
24	42.5	47	49	47	29·55	Cloudy	Ditto
25	48	49·5	41	36	29·46	Fair‡	Fair
26	41	46	49	46‖	29·89	Ditto	Cloudy§

* Rain at 11.
† Snow at 11.
‡ Generally, the day windy, rain about 2 P M. with a little hail.
§ Rain at 11.
‖ The minimum of the Thermometer at 9, gradually increasing all the night.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1810.

A mineralogical Outline of the District containing the Aluminous Schistus, in the County of York, with the entire Process practised in the Manufactory of Alum: to which is added an Analysis of the Sulphate of Alumine, and the Supersulphate of Alumine and Potash, with practical Observations and Remarks: communicated by Mr. RICHARD WINTER.

Strata.

THE stratum of aluminous schistus bordering upon the sea, presents cliffs that are in general precipitous. Their height is from 100 to 750 feet.

Sea encroaching on them.

The sea has made, and is continually making considerable incroachments upon the land, particularly to the southward of Whitby. The abbey, built in the year 656, was situate near one mile from the sea; at present the distance is not 200 yards.

Owing to decomposition.

This wasting of the cliffs is principally occasioned by the decomposition of the schistus from exposure to the atmosphere, and the subsequent action of the wind, rain, and breakers of the ocean. In walking under the perpendicular parts of the cliffs considerable danger is to be apprehended from the fragments of stones &c. continually falling down. Several instances have occurred of a very unfortunate nature, from a want of precaution, and of a knowledge of these circumstances.

The stratum 29 miles wide. Direction E. & W.

The stratum of aluminous schistus is about 29 miles in width, extending from 10 miles to the southward of Whitby to 19 miles to the northward. The general direction of the stratum is from east to west, as may be inferred from its being found in the interior of Yorkshire and Lancashire.

The sea coast most eligible for alum works.

Those places immediately upon the coast are, however, the most eligible for the erection of alum works, as they possess advantages, which it is absolutely necessary the manufacturer should embrace. The immense quantity of refuse schistus and rubbish (as the covering strata of the aluminous schistus are called) to be removed, renders it requisite to erect the works in such situations as to be able to get rid of these substances with the most expedition and the least expense. The charges for draught work is materially diminished, as the coals are brought by sea from the ports of Sunderland or Shields, and delivered at the manufactory: but in those works situate in the interior, they lie under a considerable expenditure for carriage, from which the other is exempt; so that we need not be surprised at the gradual reduction of the works in the interior from seven or eight to only one remaining.

Of the Strata reposing upon the Aluminous Schistus.

Covering strata

The strata, which are generally found covering the schistus, are alluvial soil, sandstone, ironstone, shale, and clay.

Minerals of the same species, as are found in the superincumbent strata, may be collected with the greatest facility upon the seashore with additional fossils cast ashore by the waves, after having been brought down by the rivulets, or fallen from the cliffs, and afterward washed by the action of the tide *.

Stones contained in them and found on the shore, with

The variety of agates, and fossil wood converted into agate, are numerous, and equally beautiful with those brought from Germany, and admit of as high a polish.

The cornelian, mocha, onyx, opal, and chalcedony, are found in beautiful specimens, and are sought after with avidity; they are chiefly cut into seals, bracelets, rings, and other ornaments.

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* Any person, who may be desirous of obtaining any of the mineral productions of this neighbourhood, may be supplied on application to me, by letter or otherwise.

Other substances thrown up by the waves.

Garnets are found imbedded in quartz. These occur but rarely.

A variety of flints resembling the Egyptian pebble are very common; these, when broken, present a number of concentric lines, resembling the different yearly growths in timber; and variously coloured. Sometimes fossil echini are found enclosed in the flints, others contain cubical pyrites.

Jaspers of a red and green colour, elegantly variegated, occur in abundance.

Some of the nodules of quartz contain very fine small rock crystals, regularly formed.

Mica is sometimes to be met with in the sandstone strata.

Two species of hematites, or bloodstone, red and green. The green is very scarce.

Pieces of amber are sometimes thrown ashore by the sea.

Coral is also found but of a very inferior kind.

The os sepiæ, or bone of the cuttle fish, is common, the fish itself is often cast ashore.

The madreporæ are frequently to be met with.

Puddingstone, porphyry, granite, and whinstone are found in masses of various sizes.

The metallic ores are less numerous. Iron is the most predominant.

Manganese is met with in small quantity, in the state of a black oxide.

Lead has been found crystallized, but the specimens are rare.

Sandstone strata.

The sandstone strata are found of various qualities, seldom very white, generally of a brownish red: sometimes the quantity of ferruginous matter contained in them is so considerable, as to make them very hard. Even the softer kind of sandstone, by exposure to the atmosphere, is found to grow much harder, so that it is very useful in building. The thickness of the strata varies from four yards to upwards of fifty: the general inclination or dip is to the S. W.

Springs.

Immediately under the sandstone strata are to be found

springs

springs of very good water, some of them highly impregnated with iron, united with carbonic acid gas; the acid becomes disengaged by exposure to the air, and a copious precipitate of the iron, of a dull red colour is deposited. The temperature of several of these springs is from 44° to 46° Fah. uniformly throughout the year.

Coal and jet. Under the sandstone is frequently found a seam of coal, or jet, of an indifferent quality. Sometimes these occur enclosed in the substance of the sandstone, but they rarely exceed two inches in thickness, and are of no great extent.

Clay. The clay is chiefly alluvial, or derived from the decomposition of shale. The colour is generally of a bluish gray, sometimes of a yellow ochrey colour. The thickness of the strata may be averaged at two feet.

Ironstone. The ironstone is found in loose or broken strata, from two to twelve feet thick; the quality is much inferior to those seams of iron ore found in the aluminous schistus, noticed hereafter. Its specific gravity is about 3·1. It may here be remarked, that the whole of the strata are traversed by veins, intersecting each other at right angles, in a southerly and an easterly direction. The masses, both of the schistus and sandstone, always appear in the form of solid parallelograms, occasioned by the crossing of the veins, the longest side of the solid lying between N. and S. I have noticed, that, when the stratum of clay has been uncovered for a considerable time in the summer season, on the abstraction of water from the clay it cracked in regular divisions, of the same rectangular figure as those visible in the sandstone and schistus. This observation on the regularity of the divisions assumed by alumine in drying is noticed by Chaptal (Chemistry applied to the Arts, vol II, p. 46). Perhaps the formation of the basaltic pillars may have been effected by this combination of fire and water in some gradual manner.

Of the Mineral and Fossil Bodies found in the Aluminous Schistus.

Fossils found in the schistus. A very pure native alumine is found enclosed in a nodule of stone resembling indurated clay. Several species of ammonitæ are found enclosed in an argillaceous ironstone of a double convex form. Two or three species of nautili occur, these last are rarely found in a perfect state. The belemnitæ are very abundant. The trochitæ are found, but not in great abundance. The fossil vertebræ and other bones of animals are frequently found, the form of which has been but little deranged. I found a part of the os femoris of an animal with the trochanter and the foramina very evident; the part where it was broken measured 4·2 inches in diameter, so that its length when in a perfect state may be inferred to have been at least 4 feet. The shells are numerous, of various species, and some of these are in a state of great preservation.

Naphtha is sometimes found enclosed in an ironstone of a globular form. Jet is found in abundance, frequently the bituminization is not perfect, and one part of the substance presents us with pure jet, while the other is still in the state of petrified wood: in this state it is most commonly found, in breaking up large masses of iron ore.

There is an immense quantity of red iron ore, found in strata, at the depth of about 200 feet from the top of the aluminous strata: the thickness of these seams of ore vary from about a few inches, to about 2 feet. In some situations four or five of these strata are found alternating with schistus. The specific gravity of this ore is from 3·4 to 4·2. It contains, upon analysis, from 30 to 60 per cent of iron, combined with oxigen, phosphoric acid, lime, alumine, and silex. Considerable quantities of this ore are collected, and carried down to Newcastle, and smelted at the founderies erected there for this purpose.

Sulphate of lime is found crystallized in radiated and striated crystals; but this is at considerable depths in the rocks.

Carbonate of lime is generally found crystallized, filling the veins which intersect the aluminous schistus. The thickness of these veins of crystallized carbonate of lime is generally 0·5 of an inch, and they are of considerable depth.

New variety of alum rock.

An ingenious landscape painter, and a good mineralogist, Mr. Bird, of this place, has recently discovered a new variety of alum rock, containing silex and sulphur, with oxide of iron. This rock effloresces on exposure to the atmosphere, and a sulphate of alumine is produced. The stratum is of great extent, and inestimable value. I am not permitted to point out its situation.

Aluminous schistus.

The aluminous schistus is generally found disposed in horizontal laminæ. Sometimes it exists in the form and appearance of indurated clay; in fact the whole of the upper part of the stratum resembles indurated clay, when first wrought; but by exposure to the atmosphere it suffers decomposition, and crumbles into thin layers. The upper part of the rock is the most abundant in sulphur, and the deeper they work into it, the quantity of sulphur decreases, and the bituminous substance increases, and the rock becomes more hard and slaty; so that a cubic yard of rock, taken from the top of the stratum, is as valuable as 5 cubic yards taken at the depth of 100 feet.

When a quantity of the schistus is laid in a heap, moistened with sea water, it will take fire spontaneously, and will continue to burn until the whole of the combustible materials are exhausted.

A considerable part of the cliff some years ago fell down in a situation where it was exposed to the sea at high water; in a short time afterward combustion had taken place throughout the whole extent of this small volcano, and it continued to burn for two or three years before it became extinct. Does not this fact explain the nature and cause of volcanoes? This point I am aware has been ably illustrated in the spontaneous inflammation of pyrites, the artificial volcano of Lemery, and more particularly by the indefatigable Spallanzani, and Sir W. Hamilton.

Volcanoes.

The whole extent of the aluminous strata bears evident marks of a volcanic nature. It is intersected by whin dykes, and wherever the coal strata come in contact with these dykes, the coal is charred to some distance. Wood is also found in every part of the schistus converted into charcoal. Jet appears to be some vegetable substance, that has been acted upon by considerable pressure, and some degree of heat, not sufficient to convert it into charcoal; it frequently has the appearance of a cylinder having undergone an immense pressure, and the centre filled with pyrites. The accumulation of sulphur towards the top of the strata, as if it had been sublimed—these facts seem to countenance the idea, that this is a volcanic country, where the degree of heat has not been sufficient to put the rocks into a more rapid state of combustion; or for want of the access of the sea into the interior parts of the Earth. Who can determine, that nature has not yet remaining rocks, which may become volcanoes in some future ages, when the sea has found a sufficient inlet into the bowels of the Earth.

The observations I have made with regard to the chemical nature of the schistus are merely indicative of the substances contained in it. Indeed experiments would only exhibit a conjectural, and not a real analysis of the schistus, unless a considerable number of them were made at different depths, and in various situations of the stratum.

Characters of the aluminous schistus.

The colour of the aluminous schistus is a bluish gray. Its hardness differs; at the top part of the strata it may be crumbled in pieces between the fingers, at a considerable depth it becomes as hard as roof slate. The specific gravity is about 2·48.

Alcohol digested upon it, and afterward evaporated, leaves a residuum having all the properties of petroleum.

Olive oil, digested upon the schistus, acquired a dark brown colour, most probably from bitumen.

Exposed to a red heat for a considerable time it loses 15 per cent, and assumes a whitish colour, if taken from the top of the rock, and a dull red colour, if taken at about the depth of 40 yards.

Dilute sulphuric acid was poured upon a portion of the schistus; and upon adding prussiate of potash, an abundant, precipitate of prussiate of iron was thrown down from this solution.

Ammonia precipitates a very considerable proportion of alumine, amounting to 30 per cent, in some instances.

Oxalic acid discovers the presence of lime and magnesia.

Fused with an alkali, muriatic acid precipitates a large proportion of silex.

Hence the aluminous schistus contains silex, alumine, magnesia, lime, oxide of iron, bitumen, sulphur, and water.

Of the Calcination and Lixiviation of the Schistus.

Method of working it.

The covering strata are removed previous to working the

Method of working the schistus.

the alum rock (as it is generally called). The hewing of the rock is performed with picks and javelins; and it is conveyed to the calcining place in barrows, so contrived, that the centre of gravity of the weight, is in a perpendicular line passing through the centre of the axle of the wheel; by this means the men have nothing more to do, than to keep the barrow steady, throw the weight of the substance upon the wheel, by raising the handles, and direct the barrow upon the way, which is formed of cast iron plates, 6 feet in length, 6 inches in breadth, and half an inch thick; these plates are fastened into cross pieces of wood fixed into the ground, at the end of each plate. Ten of these barrows contain one solid yard of the rock. The expenses of working the rock vary according to the facility with which it can be hewn. When the distance the rock is to be barrowed is about 200 yards, the rate for removing and hewing one cubic yard is about 6½d. It is unnecessary to state, that the price must maintain a corresponding ratio with the distance to be conveyed. The men earn about 2s. 6d. per day in the winter season, and 3s. in the summer.

The rock is poured out of the barrows upon a bed of fuel, composed of underwood, furze, &c. The dimensions of this pile of faggots is about four or five yards in breadth, and two in height; as the rock is deposited upon the fuel, it is necessary that it should be broken into small fragments, that the combustion may take place with the greater facility. When they have got about four feet in height of the rock upon the faggots, fire is set to the bottom, and fresh rock continually poured upon the pile; other piles of wood are then placed alongside of the first, and they proceed as before, adding more rock, firing the fuel, &c. This they continue, until the calcined heap is raised to the height of 90 or 100 feet, and from 150 to 200 feet in length and breadth. Some of these heaps of calcined mine (as it is now called) will contain 100,000 solid yards of schistus or rock.

When the whole heap is in a state of combustion, a considerable quantity of sulphureous acid gas is disengaged, this they endeavour to prevent, by moistening small schistus, and forming a kind of clay; with this they plaster the outside

side of the heap, this however does not prevent the escape of the gas in any degree, but it prevents the wind from penetrating, and assists in preventing the calcined mine from falling, by forming a kind of crust all over the heap; this crust is soon decomposed by the action of rain, &c.

The form of the places for calcining the rock in is badly calculated to prevent the escape of the sulphureous acid gas. If the combustion was effected in a building of the shape of a smelting furnace, immediately upon the whole of the rock becoming ignited the openings might be closed, and the gas preserved. I have ascertained by experiment, that nearly one half of the sulphureous acid gas is expelled by a red heat, continued for a considerable space of time.

Difficulty of introducing improvements.

Every suggested improvement is considered as an innovation by the illiterate, and it may be truly said, to be more easy to remove mountains than long established prejudices; the anxious manufacturer is seldom sufficiently master of his works, so as to be able to turn the scale of long established custom: and the most enlightened and scientific methods are entirely defeated, when trusted to the hands of workmen to carry them into execution.

How little melioration can be expected among a class of people, where reason has never made any impression upon the mind! I would hail the man as a true patriot, who shall endeavour to disperse this cloud of darkness from the human race.

The sulphureous acid gas, by absorbing oxigen from the atmosphere, is converted into sulphuric acid; this change is effected by means of the oxide of iron contained in the mine, and moisture. It would certainly be worth ascertaining by experiment, whether the oxide of iron combined with sulphur in burning would not yield sulphuric acid if moistened with water.

I am aware that iron has a greater affinity for oxigen than sulphur has in the fire, but in the great scale of nature she observes laws peculiar to herself: the affinities observed in the salts of the ocean are contrary to the order they appear in our tables, here, we find the small portion of sulphuric acid united to the lime; instead of forming a union with the soda, as might be inferred. Lime is found to decompose the muriate of soda. These, and other anomalies might be produced, but they are foreign to the purpose.

Making the alum.

130 tons of calcined mine will produce 1 ton of alum. I have deduced this number from an average of 150000 tons of calcined mine consumed.

The calcined mine is steeped in water, contained in pits, that usually hold about 60 cubic yards. The water thus impregnated with sulphate of alumine, called alum liquor, is drawn off into cisterns, and afterward pumped up again upon fresh calcined mine. This is repeated until the liquor becomes concentrated to the specific gravity of 1·15; or 12 pennyweights of the alum maker's weight. The half exhausted mine is then covered with water, successively, to take up the whole of the sulphate of alumine; these liquors, thus impregnated, are denominated strong liquor, seconds, and thirds.

The strong liquor is drawn off into cisterns, to deposit the sulphate of lime, iron, and earth suspended in it. In order to free the liquor from these substances, they clarify it by boiling for a short time, which enables the sulphuric acid to exert its affinities with greater energy. After running it from the pans, and suffering it to cool, the whole of the sulphate of lime, iron, superfluous alumine, and earth, are deposited; and the alum liquor is nearly pure. Where this precaution is used, the alum is much better in quality, and almost entirely divested of the sulphate of iron. This method is only practised at some of the works, owing to the additional quantity of fuel required, and consequently increased expense.

The liquor in this state is carried by means of pipes, or wooden gutters, into leaden pans. These pans are made of sheet lead (cast by the workmen in the alum house) 10 feet long, 4 feet 9 inches wide, 2 feet 2 inches deep at the hinder part, and 2 feet 8 inches at the front end: this difference is allowed to give a rapid current in running off.

A quantity of mothers is pumped into the pans every morning; and, as this evaporates, the deficiency is supplied with fresh alum liquor, every two hours, or, as the liquor in the pans becomes more concentrated, the additions are made more frequently. It is necessary to keep the pans continually boiling, otherwise the superfluous alumine and sulphate of alumine, deprived of its water of crystallization, would be precipitated, and the pans melted, from the crust formed between the liquid and the lead.

Each pan will produce upon an average 4 cwt. of alum daily, and the consumption of coals will be about 18 bushels Winchester measure.

The liquid contained in the whole of the pans is run off every morning into a vessel called a settler, at the same time a quantity of alkaline lee is brought along with the boiling liquor, prepared either from kelp, soapers lees, (generally called black ashes) or muriate of potash, of a specific gravity from 1·037, to 1·075. The alum maker having previously ascertained the specific gravity of the liquid in his pans, estimates the quantity of alkaline lees to be added, necessary to reduce the liquor from the pans from the specific gravity of sometimes 1·45 or 1·5 to 1·35.

The liquor then stands in the settler about two hours, that it may deposit the sediment it may contain, when it is run off into the vessels (or coolers) to crystallize.

If the alum maker should be below, or equal to the specific gravity of 1·35, in mixing the alkaline lee and liquor, there is nothing more to be done. If he exceed this specific gravity, he then adds urine in the coolers, until the liquid is reduced to 1·35. It is then agitated to combine the heavy and light liquids, and then left to crystallize. It must be observed, that at a greater specific gravity than about 1·35, the liquor, instead of crystallizing, would present us with a solid magma resembling grease.

After standing four days, the mothers are drained off, to be pumped into the pans again the succeeding day. The crystals of alum are conveyed into a tub, where they are washed in water, and put into a bin, with holes in the bottom, to allow of the water draining off from the alum. They are then removed into a pan (twice as large as the common leaden pans), and as much water added as is found requisite to dissolve the whole of the alum when in a boiling state: the moment this is effected, the saturated boiling solution is run off into casks. These casks should stand about 16 days; as they require this time to become perfectly cool, in the summer season. The casks are then taken to pieces, and a hollow cask of alum is produced; it is then broke into, and the whole of the saturated solution of alum (called tun water) is removed back into the pans, to go through the process anew.

This last process is called roching. The outside of the cask of alum is now to be cleared from dirt, and the sediment which is deposited at the bottom. It is then broken up into masses ready for the market.

Practical observations and remarks upon the foregoing processes.

Method of ascertaining the specific gravity.

The method pursued by the alum makers to find the specific gravity of any liquid is capable of considerable accuracy. A bottle is procured, that will contain about ¼ of a pint. The narrower the neck, the more accurate will be the results obtained by it. This bottle is balanced in a pair of sensible scales, we will suppose it to weigh 1000 grains, it is then filled with distilled water, and carefully dried with a cloth; now allowing the water to weigh 2400 grains, this last number is divided into 80 parts or pennyweights, and we have 30 grains corresponding to one pennyweight; this they subdivide into ¼ and ½. Hence we may ascertain the relative specific gravity of any liquid. 1 pennyweight is equivalent to 1·0125, and 80 pennyweights to 2·0. Care however is necessary, to have a counterweight of 3400 grains, equal in weight to the water and bottle together, which must always be put into the scale, along with the other weights, in operating. This was formerly a great secret among the alum makers, and they sold the method at a high price, or handed it down to their children as an hereditary possession.

Improvements suggested.

Considerable advantage might be derived to the manufacturer, by reducing the size of the fire places, and erecting iron doors, to prevent a current of air passing over the fire, instead of entering by the ash pit: a very material saving of fuel would arise from adopting this method.

A very material errour is committed, by concentrating the liquor in the pans to near the specific gravity of 1·5, and then reducing it again to 1·35: this method obliges them to evaporate a very unnecessary quantity of water.

The alum liquor is frequently brought into the pans as low as 1·09; when by repeatedly bringing the liquor over fresh calcined mine, it might be concentrated to 1·25, or more. I will mention an instance where the expenditure in evaporating liquor was more than £3 10*s.* daily; when at the same time this liquor might have been concentrated to an equal degree, by repeatedly pumping the liquor upon fresh calcined mine, at an expense of not more than 9*s.* in the same time; here there was a loss of £3 1*s.* daily.

In using black ashes, or kelp, a considerable quantity of charcoal is dissolved in the alkaline lee; this charcoal is precipitated on adding a small quantity of the solution of sulphate of alumine, but is redissolved again by adding the solution in excess.

This charcoal then contaminates the alum, and decomposes a quantity of the sulphuric acid: therefore, it must appear conclusive, that whatever alum is made with muriate of potash alone will be far superior in quality, while the produce will be greater in quantity.

It might be supposed, that urine was a necessary ingredient in the making of alum; but the fact is, it merely hides the ignorance of an alum maker. Having no determinate rule to guide him, in reducing the liquor from the pans, should he chance to exceed the specific gravity of 1·35, he adds urine, or some such light fluid, to bring the liquor as near as possible to this density. The alum works, that approach the nearest to the true chemical principles, are those of the Right Hon. Lord Dundas, and Messrs. Baker and Co. They use no urine in these works—the alum liquor is always clarified previous to its being used—they use no alkali generally, but crystallized muriate of potash—greater economy is observed in the consumption of fuel; and the result is a product of alum considerably larger in a given time, and of better quality, than can be produced by the works established upon the old plan.

Remarks on the alkalis employed in the manufacture.

The kelp used is obtained by burning the sea wrack in kilns, at a great number of places upon the coast of England, Scotland, &c. It is a very inferior alkali in an alum manufactory. It contains about 47 of soluble salts, and 53 of charcoal, sand, and earth. The salts are muriate of soda, soda, and sulphate of soda.

The refuse of the soap boilers' lees are burnt in a kind of oven, and sold under the name of black ashes. The composition of these ashes is about 90 of soluble salts, and 10 of charcoal and earth, the salts contain muriates of soda and potash, sulphate of potash, and muriates of lime and magnesia.

I have always found great difficulty in producing alum by the muriate of soda, and never could form alum in any way by means of pure soda.

The muriate and sulphate of potash are the only alkalis that can be used to advantage in the composition of alum.

I have made comparative experiments to ascertain the quantity of the different alkalis it would require to produce 100 tuns of alum. The following are the results:

42 tuns of muriate of potash will produce 100 tuns of alum,
31 ditto of black ashes .... 100 ditto,
73 ditto of kelp .... 100 ditto.

The alkalis are considered as in the state, in which they are found in commerce.

Analysis of sulphate of alumine, and supersulphate of alumine and potash.

Analysis of sulphate of alumine.

I have been generally disappointed in analyzing alum on finding my results at variance with those of so many eminent chemists. It appears, that the errour has existed in their different estimations of the composition of sulphate of barytes. It seems, that allowing about 33 per cent of acid is very near the truth*. By taking it in this ratio, the acid used, and the quantity of alum produced upon a large scale nearly correspond.

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* According to some very careful experiments made by Mr. Arthur Aikin, see Journal, vol. XXII, p. 301, it is nearer 34. He makes it 33·96; and according to Klaproth it is 33·55. - See also an Analysis by Mr. James Thomson, vol. XXIII, 174; and another by Berthier, ib. p. 280: both of whom make it at least 33. C.

I know of no experiments, that have been made to ascertain the composition of sulphate of alumine, except Bergman's. I believe Vauquelin has done the same, but as I have not seen his paper, I cannot speak to that effect. Bergman states the composition

Sulphuric acid	50
Alumine	50
	100

From a solution of pure sulphate of alumine in water I precipitated the acid, by means of the muriate of barytes added in excess. The precipitate, after having been carefully washed upon a filter, was exposed to a white heat for some time. The quantity of acid inferred from the sulphate of barytes obtained, was 287 grains.

To an equal quantity of a solution of sulphate of alumine, ammonia was added to saturation; the precipitate, after washing upon a filter, was exposed to a white heat for an hour, and weighed 209 grains. Therefore sulphate of alumine is composed of

Sulphuric acid	57·8
Alumine	42·2
	100·0

Analysis of alum.

Analysis of supersulphate of alumine and potash.

Exp. 1. To 10000 grains of a very pure crystal of alum, dissolved in rain water, was added muriate of barytes, until no farther precipitate took place. The sulphate of barytes, after having been well washed upon a filter, was exposed to a white heat for some time, it weighed 3359 grains.

Exp. 2. To 10000 grains (of a part of the same crystal, as used in the preceding experiment) in solution, was added ammonia, until the precipitation was found to be complete. After washing and filtering the alumine, it was exposed to the heat of a blast furnace, and found to weigh 1096 grains.

Exp. 3. The remaining solution of the 2d experiment, after boiling quicklime in it to take the acid from the ammonia, was evaporated to dryness, and afterward exposed to a red heat. The potash, or soda, thus produced, weighed 858 grains.

I repeated the analysis twice, the results, after taking every precaution, are as under:

	1st Exp.	2d Exp.	Mean of the two.
Sulphuric acid	33·59	33·10	33·34
Alumine	10·96	11·81	11·38
Soda or potash	8·58	9·74	9·16
Water	46·87	45·35	46·12
	100	100	100.

In finding the quantity of water contained in crystallized alum, by exposure to a red heat, an uncertain product will always be given, arising from the degree of heat employed in the desiccation of the alum. At a white heat a very considerable proportion of acid will be expelled, as well as the water.

In the production of alkali from the alum, I have called it soda, or potash. I did not institute any experiments to ascertain the quality of the alkali, but as nothing but kelp and black ashes had been used in the fabrication of the alum, it is evident the alkali must have been soda with perhaps a small proportion of potash. So that in reality we have a fifth variety in addition to the four described by Dr. Thomson.

It remains then of some importance to the consumer to ascertain the difference between alum made with potash, and that in which a salt of soda is used.

I have received considerable advantage from the very able memoir of Messrs. Thenard and Roard, as inserted in your Journal, vol. 18, page 276.

Had these philosophers adopted 33 per cent of acid in the sulphate of barytes, instead of 26, as is stated in their paper, our results would not have been very different. By correcting their statement of the composition of alum, according to the estimate of sulphate of barytes, which I have used, we shall find it to be

Sulphuric acid	33·05
Alumine	12·53
Potash	7·90
Water	46·52
	100.

Whitby, 10*th March*, 1810.

II.

Tools to answer the Purpose of Files and other Instruments, for various Uses, made of Stone-ware. By G. CUMBERLAND, *Esq.*

To Mr. NICHOLSON.

SIR,

Clay employed to abrade hard bodies.

TO some men, but not to you, will it appear a trifle, because very obvious on reflection, to have applied so soft a substance as clay to the purpose of lograting the hardest bodies; neither should I perhaps have ever thought of such an application in the form I now use it, had I not found, in shaping some substances, that the wear of my steel files was rather expensive.

Tools made of it for this purpose.

It then first occurred to me, in ranging in thought after a remedy, that, as our stone-ware is so hard as to blunt our files, files might be as well made of our stone ware. This was about two years ago, and the first use I made of the suggestion was, to fold up in muslin, cambrick, and Irish linen, separate pieces of wet clay, forcing them by the pressure of the hand into the interstices of the threads, so as on divesting them of the covering to receive a correct mould. These I had well baked, and immediately found I had procured an intire new species of file, capable even of destroying steel; and very useful indeed in cutting glass, polishing, and rasping wood, ivory, and all sorts of metals.

Uses to which they may be applied.

The ease with which I had accomplished my purpose, as is too often the case, made me content myself with the use of my own discovery, or at most giving away a few specimens as files for ladies nails of peculiar delicacy: but having since reflected, that in glass grinding (the stones for which come from the North, and are very expensive) in flatting metallic mirrors, laying mezzotinto grounds, and a number of operations that require unexpensive friction, these stone-ware graters, if I may so call them, as not being of the exact shape of files, may ultimately become very useful. I take a pleasure in furnishing you with a description of my method of applying this substance, accompanied with a specimen or two of a portable size, that you may the better be able to judge of their value to the arts, which to me, the more I reflect on them seem the more important; as in all operations of grinding a great deal of manual labour must first be bestowed on the tool, whereas here we may mould ours in an instant, if we use a press, as in pipe making, and the expense is infinitely inferior to that incurred in constructing even the cheapest file or logrator.

Some of their advantages.

I am, Sir,

Your most obedient humble servant,

Bristol, Feb. 10th, 1809. G. CUMBERLAND.

Perhaps they might be usefully employed for speculums.

P. S. I have not yet tried it, not having the means just now at hand, but if a good parabolic reflector were to be impressed with a mass of stone-ware clay covered with muslin, so as to make several casts of different degrees of fineness, we might this way acquire tools, that would greatly lessen the expense of the operation of grinding; but much would depend on care in baking. Our stone ware warps but little ever.

ANNOTATION.

This ingenious invention promises to be of considerable use in the arts. The abrasion of surfaces is performed either by a toothed tool, as in filing, rasping, &c.; or by a grinder, in which cutting or hard particles are bedded with considerable firmness in a softer mass; or by scowering, polishing, &c., in which hard particles are more or less slightly retained in a soft or tenacious substance. Mr. Cumberland's instruments appear to promise great utility in the first and last of these processes; that is, they may be used either with or without a fretting powder. There are however many objections to their being used to grind speculums; not only with regard to the intended figure, but the nature of the material.

W. N.

III.

Remarks on Meteorology. In a Letter from THOMAS FORSTER, *Esq.*

To Mr. NICHOLSON.

SIR,

Accurate observations of clouds important to meteorology.

I Was much pleased with the perusal of your correspondent Dr. Bostock's letter on meteorology, p. 196, and particularly with his plan of a diary. I am of opinion, with him, that an attention to the several modifications of clouds, and the changes of weather which succeed them respectively, is of great importance in meteorology; and it is much to be wished, that more accurate and frequent observations of this sort were made. A want of a nomenclature capable of expressing clearly all the different modifications of cloud has been the reason, why this branch of the science has been hitherto so little attended to by meteorologists, but your correspondent does not seem to be aware, that this is no longer a desideratum in meteorology. The ingenious Mr. Luke Howard, of Plaistow, published a few years ago a short treatise on the various modifications of clouds, in which he has determined their distinctive characters, and shown the manner in which they are probably formed. The substance of this pamphlet may be found in Rees's Encyclopedia under the word *cloud*. In a meteorological diary, which I keep, I always note down the different modifications, placing in a parallel line all those which appear in the sky at once. I

Nomenclature of them by Mr. Howard.

Plan of a meteorological diary.

would

would recommend the adoption of the following plan for a meteorological diary. It should consist of twelve columns headed as follows. 1st column, day of the month; 2nd and 3rd the maximum and minimum of the thermometer; 4th and 5th, ditto of barometer; 6th, the quantity of rain which falls in the course of the day; 7th, the quantity of evaporation in square inches; 8th, the state of the hygrometer; (de Luc's is perhaps the best) 9th and 10th, the direction and force of the wind; 11th, the modifications of cloud; and the 12th should be reserved for the register of occasional meteorological phenomena, such, for example, as thunder storms, meteors, &c.

The insertion of these hints in your truly scientific Journal will oblige

Yours, &c.

No. 6, St. Helen's Place, THOMAS FORSTER.
Mar. 8th, 1810.

IV.

On expectorated Matter; by GEORGE PEARSON *M. D. F. R. S.*

(Concluded from Page 229.)

SECT. III. *Agency of Alcohol of Wine.*

Opaque viscous matter desiccated and digested in alcohol.

1. (*a*) TWO thousand five hundred grains of desiccated expectorated matter of the fifth sort, sect. I, 5, being the one twentieth of 50000 grains of matter previously to evaporation to dryness, were digested in four pints of alcohol of spirit of wine, of the specific gravity of 815, water being 1000. The mixture was exposed at a temperature from 58° to 68° for a month, during which it was frequently shaken. A tincture of the colour of red port wine was then decanted from off a blackish sediment. By means of a press, two ounces more of the tincture were obtained.

Residuum.

(*b*) The undissolved residuary matter being exsiccated weighed 130 grains less than before digestion. On exposure to the air, it remained dry, but it became more flexible. It no longer emitted ammonia on trituration with lime.

(*c*). The tincture thus obtained was distilled readily till there remained about five ounces measure in the retort, and what remained seemed to be chiefly water instead of spirit, with such a quantity of matter dissolved in it, as not to afford liquid by distillation, without frequently spirting into the receiver. The residuary liquid was therefore evaporated to the consistence of a soft resinlike extract of a black colour; which had a salt with bitter taste.

The tincture distilled.

The distilled liquid had a peculiar pungent smell, but not that of ammonia, and it neither reddened turnsole paper, nor rendered violet cloth green.

Distilled liquid.

(*d*). The resinlike extract (c) weighed 140 grains. It was semitransparent—dissoluble in water, but not coagulable in boiling water—it grew softer on exposure to air—it was uncrystallizable—it betrayed no signs of alkalescency or of acidity, except giving turnsole paper a reddish hue—under the blowpipe it burnt like matter from animals, and afforded a fused globule, which indicated muriate of soda, and a large proportion of potash, deliquescing very speedily—with lime it emitted the smell of ammonia—with phosphoric, and also with tartaric acid, on being heated, an acid smell was perceived, which I at first mistook for acetous acid; but I soon found that no such acid was present, not being able to detect a trace of any acid in the distilled liquid from these mixtures—on the addition of acetite of lead, a very copious precipitation of fawn-coloured sediment instantly took place, with the smell most distinctly of apples. The decanted liquid of this mixture was found to be chiefly acetite of potash. On dropping diluted sulphuric acid upon the fawncoloured sediment, it constantly emitted the smell of apples. I could not, however, satisfy myself, that the small quantity of liquid decanted from off this sediment contained a kind of vegetable acid for the first time apprehended in the fluids of animals; because, first, the quantity of product I possessed was so diminished by many experiments, that I was unable to make what I considered to be decisive trials. Secondly, because in

Extract.

Supposed acid.

Perhaps mistake.

in subsequent processes I failed in producing the same apple-smelling liquid. Hence I considered, that the supposed acid, which had some of the properties of the malic, only occurred occasionally; or that I had been deceived, and that I had procured nothing more than a little of the acid employed for the decomposition, disguised by mixture with the subject of the experiments. The fawn-coloured precipitate was, no doubt, chiefly muriate of lead. Still the experiments fully demonstrated the presence of potash neutralized, either by an acid destructible by fire and dissoluble in alcohol, but hitherto not disunited from animal oxide, or that an oxide of animal matter alone neutralizes the potash, as will be manifested by the evidence of experiments to be related.

Potash and muriate of soda.

(e). Forty-five grains of the residue (*c*) which had been dissolved in alcohol, being burned in a platina crucible, yielded chiefly potash, and half its quantity of muriate of soda.

(*f*). Twenty-five grains of the residue (*c*) were boiled with successive portions of nitric acid, till the oxide of animal matter was decompounded, and carried off in the state of gasses; and then deflagration took place, leaving subcarbonate of potash with muriate of soda and charcoal.

Component parts of the extract.

According to a computation, the 140 grains of resinlike extract (*c*, *d*) consisted of twenty-eight grains of potash, and eighteen grains of muriate of soda, with an inappreciable quantity of ammonia, and perhaps phosphoric acid, beside the oxide of animal matter, and possibly an acid of an unknown kind.

Matter not soluble in alcohol.

(*f*). The undissolved matter (*b*) was burned in a platina crucible. It afforded a residue, which I could not render fluid by fire, but only of the consistence of paste? On cooling, it was a brittle gray mass weighing fifty-six grains, somewhat salt and gritty to the taste. It consisted of muriate of soda, and phosphate of lime, about twenty-three grains of each,—of potash four grains—of fused matter, which by long boiling in muriatic acid yielded phosphate of lime, muriate of lime, and utterly indissoluble vitrified matter with traces of magnesia, oxide of iron, and a sulphate.

2. Four

Opaque ropy matter digested in alcohol

2. Four thousand grains of expectorated matter of the third kind, page 219, sect. II, 7, were added to two pints of rectified spirit of wine. By agitation the spirit became at first milky, but presently it grew clear; little curdy masses appearing, which fell to the bottom as a sediment, being in bulk about one fourth of that of the added expectorated matter.

Extract.

After a month's digestion, the filtrated liquid, on evaporation, afforded a dry extractlike residue, weighing sixty grains. It grew moist by exposure to the air, but not when kept in close vessels. It consisted of the same ingredients, but in very different proportions, as the residue from distilling and evaporating the tincture, page 224, Sect. III, 1, the present residue containing a much larger proportion of muriate of soda, and oxide of animal matter.

Successively treated in the same manner.

Successive digestions of the same matter afforded less and less saline residue, but nearly the same proportion of oxide of animal matter for three times, but then no saline matter was afforded, but merely animal matter. The residues of the evaporated tinctures of the subsequent digestions did not, like the first, grow moist, but only softer; and the oxide of animal matter from each of them was no longer coagulable, although afforded by dissolution of coagulated matter. It appeared that the animal oxide was of one kind only, and that the whole of it might be dissolved in alcohol, and thereby become uncoagulable, and more easily dissoluble in every kind of menstruum.

Animal oxide,

Mixture of equal parts.

3. If a large proportion, namely, two parts of expectorated matter be mixed with two parts of rectified spirit of wine, the matter is in great part, at least, coagulated, but the spirit is rendered milky. The same is true with regard to other menstrua. The reason is obvious. The coagulation is produced by the separation of water from the animal oxide of the expectorated matter, by the attraction of the alcohol, or of acetous acid for the water; but if there is not a due proportion of spirit or acid, the oxide of animal matter retains so much of the water, as to render the liquid milky. A person accustomed to these experiments may determine pretty exactly, by means of them, the proportion of water in the expectorated matter, it being directly

Coagulation.

Proportion of water determined.

rectly as the quantity of spirit or acid requisite to produce entire coagulation in a clear liquid; and the proportion of coagulable animal oxide is, within certain limits, inversely as the quantity of spirit requisite for coagulation.

Opaque ropy matter digested in ether.

4. Sulphuric ether, being in many properties analogous to alcohol of wine, I digested three hundred grains of exsiccated matter of the third kind, page 219, in four ounces by measure of this menstruum for a month, in a warm room, during which the vessel was often agitated. Three ounces of a black tincture were thus procured, which, on distillation to dryness, afforded sixty-five grains of soft extract. This extract became a little moist on exposure to the air, and was then a little viscid. It burnt with flame like oil to the state of charcoal; and this again, on burning, only left two grains of residue, which consisted of muriate of soda, with indications of alkali, and phosphate of lime.

Residuum.

The undissolved residue also remained soft, and could not be made brittle by evaporation. After inflammation and incineration, the usual products were obtained as from matter which had not been digested. This menstruum had therefore dissolved abundantly the oxide of animal matter, and but a small proportion of the saline and earthy parts.

Apparently uniform expectorated matter not of the same consistence throughout

4. Apparently uniform expectorated matter is not of the same consistence through the whole mass; for a few drops of the opaque kind being shaken in half a pint of rectified spirit of wine, the whole does not dissolve, but it is broken into small curdy particles, which fall as a sediment in a clear liquid, seemingly about one fourth of the original bulk of the matter.

SECT. IV. *With Water.*

2d and 4th kinds treated with cold water,

1. None of the kinds of expectorated matter are readily diffusible through cold water, except the second and fourth, page 219 and 221; and by agitating them some fibrous pieces are usually detached; also on inspecting the water after this diffusion, it appears full of small masses, or motes. On standing, these suspended masses become a sediment; which is the case, although the proportion of expectorated matter be exceedingly small to that of the water.

2. When

and very hot water.

2. When very hot water is used, namely, that of a temperature from 190° to 210°, a still greater number of motes are perceivable, especially with a lens, and the water is rendered milky.

Other kinds with cold water.

3. Brisk agitation is required, for a due length of time, to diffuse the other kinds of expectorated matter through cold water; but a great number of fibrous and membranous pieces appear, the form of which cannot be destroyed, or only partially, by shaking, in almost any proportion of water. Three drops of ropy and opaque matter were shaken in half a pint of distilled water. About one half of them was diffused; the rest of them was in the form of small fibrous, leafy, and irregular figured motes; which, on repose, formed a sediment, and remained in that state three months; although in that time the water became highly fœtid, and sometimes in this experiment the sides of the vessel were tinged black.

And with hot water.

4. Agitation of these sorts of expectorated matter (3), in a large proportion of water at the temperature of 170° and upwards, produced a greater degree of milkiness, and a greater number of small masses, which could not be dissolved by long shaking. Putrefaction did not take place so soon in these mixtures, as in those in cold water.

Mechanical mixture.

5. If the proportion of the last mentioned kinds of expectorated matter be two or three parts to one of cold water, or under the temperature of coagulation, a uniform mixture may be produced by violent agitation, the water being entangled by the viscidity of the matter, rather than chemically united.

This boiled.

6. On boiling the mixtures (5), a great part of the expectorated matter is separated in a curdy form from a milky liquid.

Action of reagents.

7. If less than two grains of expectorated matter were diffused through five hundred grains of water, no evident precipitation was occasioned by tannin; while with one grain of isinglass jelly, or white of egg, or of serum of blood dissolved in five hundred grains of water, there was an evident precipitation with this reagent.

8. I could arrive at no useful conclusions, for the distinction of expectorated matter from other coagulable, or

from any gelatinous substances by comparative trials with muriate of tin, nitro-muriate of gold, oximuriate of mercury, acetite of ceruse, and acetite of litharge.

Sect. V. *Agency of acetous Acid.*

Ropy opaque matter treated with distilled vinegar.

1. Twenty ounces of ropy opaque matter, by being shaken with ten pints of distilled vinegar, were so broken into a fibrous or even vascular form as to exhibit an organized appearance, the bulk being reduced to at least one third of the ropy matter. By repeated agitation and long digestion, the coagulated masses were broken into smaller pieces, but did not appear to be farther contracted in bulk, or to dissolve. With some parcels of matter the vinegar preserved its transparency, with others it became wheylike, the matters being deposited in a curdy state. The mucilagelike expectorated matter, or this mixed with the other kinds, afforded wheylike, or more or less turbid liquids with vinegar.

Liquid distilled

2. (*a*) The decanted liquid, and the liquid obtained by pressure of the sediments of the last mixture (1), being distilled to about one eighth, the remainder was evaporated to the consistence of a thick extract. The distilled liquid did not appear to have received any impregnation, except what had altered a little the odour. This extractlike residue amounted from one forty-fifth to one eightieth the weight of the expectorated matter, according to the kind of this substance. It varied also according to the proportion of the matter to the acid menstruum.

Extract.

Digested a 2d time,

(*b*). The residue (2. *a*) just mentioned, after a digestion a second time, in the same quantity of acid, afforded a smaller quantity of extractlike matter than before.

a 3d,

(*c*). The third digestion afforded still less of this substance.

4th, 5th,

(*d*) The fourth and fifth digestion gave somewhat less than the immediately preceding one.

and 6th,

(*e*). The sixth digestion yielded nearly the same proportion of extractlike matter as the fourth and fifth.

Insoluble matter incinerated.

3. The undissolved matter, after these repeated digestions in vinegar (1, 2), being exposed to fire in a platina crucible, first flamed and partially melted; then became

apparently charcoal, which burned away to the state of a brown earthlike substance, scarcely $\frac{1}{120}$ of the weight of the substance subjected to fire, and not above $\frac{1}{1500}$ of the expectorated matter by which it was afforded. It consisted chiefly of phosphate of lime, with indications of carbonate of lime, of a sulphate, of a muriate, of silica, or at least vitrified matter, and of oxide of iron.

Extract partially deliquescent.

4. The extractlike matter, from the first digestion of the expectorated matter (2, *a*), by exposure to the air, in a few days partially deliquesced, affording no signs of alkalescency, but having a peculiar salt taste.

Treated with nitrous acid.

(*a*). A little of this deliquescing part being burnt to dryness, with a large proportion of nitrous acid, on beginning to be ignited, it deflagrated, leaving a blackish saline residue; which soon deliquesced, and being lixiviated, it precipitated supertartrate of potash with tartaric acid, and gave a reddish precipitate with nitro-muriate of platina. The residue also contained lime, for the dissolution in acetous acid afforded oxalate of lime, on the addition of oxalate of ammonia.

Digested in alcohol.

(*b*). This extractlike matter, (2, *a*), by digestion in rectified spirit of wine, gave a blackish tincture, which, being decanted and evaporated, left a residue. This became quite liquid after twenty-four hours exposure to the air. It consisted chiefly of acetite of potash, with an inappreciable portion of muriate of soda, and ammonia neutralized, probably, by phosphoric acid; beside uncoagulable and ungelatinizable oxide of animal substance.

Residuum.

(*c*). The undissolved matter by spirit of wine, just spoken of (*b*), after expression, being desiccated, it remained in a solid state after exposure to the air, only growing a little soft in four weeks time. By combustion, it afforded a difficultly fusible ash, which after fusion was found to consist chiefly of phosphate of lime, muriate of soda, with a little potash; a sulphate, traces of iron, and vitrified matter, which probably contained silica united to the other substances manifested in this fused mass.

Extract from the 2d digestion.

5. The extractlike matter, by acetous acid on the second digestion (2 *b*), grew soft, but did not deliquesce on exposure to the air. It was found to differ from the matter ob-

tained by the first digestion in the same menstruum, in containing a much smaller proportion of potash and muriate of soda, as well as of neutralized ammonia.

From the 3d.

6. The extractlike matter, from the third digestion in vinegar (2 *c*), differed from the former, in containing a still much less quantity of the salts just mentioned.

From the subsequent.

7. The fourth and subsequent digestions (2, *d*, *e*) afforded extractlike substances, which contained scarcely any thing but a very small proportion of earthy phosphates, and indissoluble vitrified matter, produced by incineration and fusion. It did not appear, that the oxide of animal matter, dissolved by the distilled vinegar in all the preceding digestions successively, was of different kinds; but it appeared, that its coagulable property was destroyed by dissolution in this menstruum. Accordingly, there is no reason to believe, that the whole of this oxide is not dissoluble in the acid here employed, although the requisite proportion may decrease after each digestion, within certain limits.

Opaque ropy matter treated with vinegar.

8. A few drops of opaque ropy matter being agitated in half a pint of vinegar, a number of fibrous masses appear, apparently one fourth or one fifth of the bulk of the matter added; and these fibrous forms subsist, notwithstanding continued agitation, totally disappearing only in consequence of long digestion in successive large quantities of this acid.

SECT. VI. *Some Experiments with different Objects.*

Potash neutralized by animal oxide.

1. To produce a synthetic proof, that potash may be neutralized by oxide of animal matter, I triturated ten grains of the exsiccated and coagulated part of expectorated matter, freed from all saline substance, with pure potash gradually added, and a little water. Several grains were in this way united, without any effect being produced by the compound on turmeric paper. More alkali was added till the compound barely manifested the existence of alkali to the test just mentioned. It was then digested in spirit of wine, to which it imparted a deep brown colour, and the tincture being distilled, it afforded a dry extract, which grew moist on exposure to the air, but scarcely affected turmeric paper.

On incineration, however, the alkali was denuded, and fusion was easily produced.

Animal oxide digested in alcohol.

An equal portion of the animal oxide, of the same parcel as in the last experiment, was digested in spirit of wine, in the same circumstances as this oxide united to potash. It imparted no colour to the spirit, and the extract obtained was in smaller quantity, than in the preceding experiment. Being evaporated to dryness, the residue did not grow moist, but it became a little soft on exposure to the air. Being exposed to fire, it left an inconsiderable proportion of infusible residue, with barely traces of alkali and muriate.

Examination for acid.

2. To determine, by a more satisfactory experiment than a preceding one, whether or not acid was united to the potash and evaporable, ten ounces of watery liquid, which separates from the curd on boiling expectorated matter, were evaporated to the consistence of a thin extract. This matter indicated neither acid nor alkali in a disengaged state, but it was ascertained to contain a large proportion of potash combined; and an acid smell was perceived on heating it with phosphoric or tartaric acid. Ten drops of liquid phosphoric acid were mixed with four hundred grains of this extractlike matter, and at a low temperature it was subjected to distillation to become almost a dry substance; but no acid could be detected in the liquid which came over, nor did this dry substance indicate any acidity to the usual reagents—neither on exposure to the air did it, as before the addition of acid, grow moist. Phosphoric acid was farther added, till it became sensible to the test of turnsole; but neither by elutriation nor distillation could any acid be obtained, except a small portion of the phosphoric acid by elutriation, the rest having united to the potash.

Very little ammonia.

3. To furnish an estimate of the proportion of ammonia, I subjected to distillation a mixture of a pint of expectorated matter of the fifth kind, page 222, with three ounces of well burnt lime, but I could not reckon the ammonia in the distilled liquid at more than two cubic inches, or less than half a grain in weight.

SECT. VII. *Conclusions.*

Various kinds of expectorated matter differ only in the proportion of the ingredients.

1. From the preceding experiments and observations, and from others, which I might have related, it does not appear, that the various kinds of expectorated matter, page 217, differ in the ingredients of their composition, but merely in the proportion of them to one another.

These albumen, and water impregnated with salts.

The albumen in the state of oxide.

2. It has been shown, that expectorated matter consists of coagulable, or, as it is also now frequently termed *albuminous* animal substance, and of water impregnated with several saline and earthy bodies—that the largest proportion of the animal substance, which may justly be called an oxide, amounts to one twelfth, and in some very rare cases to one tenth of the expectorated matter, reduced to a brittle state by evaporation; and that the smallest proportion of this oxide, in rare instances, amounts to one forty-fifth of the expectorated matter; but that the usual proportions of it vary between one twentieth and one sixteenth of this coagulable oxide to the evaporable water, that is, between five and six per cent of the expectorated matter.

Impregnating substances.

3. The impregnating substances have been shown to be muriate of soda, varying commonly between one and a half and two and half per 1000, of the expectorated matter—potash varying between one half and three fourths of a part per 1000—phosphate of lime about half a part of 1000—ammonia, united probably to the phosphoric acid; phosphate, perhaps of magnesia; carbonate of lime; a sulphate; vitrifiable matter, or perhaps silica; and oxide of iron. But the whole of these last six substances scarcely amounting to one part in 1000 of the expectorated matter, it would be useless to estimate the proportion of each of them. It is very probable, that the proportions and quantities of these ingredients vary much more than now represented in different states of disease and health*. It is very probable also, that some of the ingredients may occasionally be absent, and others of a different kind be present, agreeably to the different states, on different occasions, of the other secretions.

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*In one case, the opaque expectorated matter in a pulmonary consumption, having been exsiccated to brittleness, became almost liquid after a night's exposure to the air.

Differences of consistence.

4. It is manifest, that the different states of consistence of expectorated matter are owing to the proportion of albuminous or coagulable oxide, but I purposely avoid giving an account of the different conditions of health, on which the difference of consistence depend.

The thickest contains the least salts.

5. The thicker the matter, the smaller I commonly found the quantity of saline impregnation. Hence in sudden and copious secretions of the bronchial membrane, the matter is asserted to be salt, and to feel hot. In such instances the proportion of coagulable matter was small, but that of the saline impregnations, particularly of the muriate of soda, and neutralized potash, so great, that the exsiccated expectorated substance tasted very salt, and presently grew moist, or even partially deliquesced; but the opaque ropy or puriform matter afforded a much larger proportion of exsiccated residue, which was but slightly salt, and generally only became soft on exposure to the air. This property of growing moist depends on the potash.

Neutralized potash in all the human fluids.

6. Each of the human fluids, according to my experiments, contain neutralized potash; at least, this is the fact of the blood, dropsy fluid, pus of abscesses, and pus secreted without breach of surface; the fluid effused by vesicating with cantharides; the urine; and in course in the very abundant secretion from the nose by catarrh. The alkali being united to oxide of animal matter in these fluids, it is easily demonstrable.

Expectorated secretion more saline than expectorated pus.

7. Although I think I have discovered many properties, by which expectorated secretion may be distinguished from expectorated pus, I shall not speak of them, on this occasion, further than just to observe, that the saline impregnation of pus, particularly that of potash, and muriate of soda, is in very much less proportion than in expectorated secretion; and hence it does not become moist after exsiccation, on exposure to the air.

No uncombined soda in the fluids.

8. It has been, I believe, uniformly asserted, that the circulating and secreted fluids are impregnated with soda; that it is especially in the matter secreted by the bronchial membrane. The experiments of others must confirm or disprove mine. It seems, however, much more reasonable, that the human fluids should be found to contain potash than

soda, united to some oxide or destructible acid; because the former alkali is daily introduced with the vegetable food, and with the drink of fermented liquor; and it is as little likely to be destroyed, as the muriate of soda also induced in the very same way. But our food and drink do not, commonly at least, contain the soda united to a destructible acid, or an oxide.

Expectorated matter not a mucous fluid.

9. It is plain, from the preceding experiments, that expectorated matter belongs to the class of coagulable fluids, and not of gelatinizable, or, as commonly asserted, mucous fluids. It differs from the coagulable fluid, serum of blood, in forming a much thicker fluid with a much larger proportion of water: for serum, and also the water of blisters, are quite liquid, although they afford on exsiccation from one twelfth to one eleventh of their weight of brittle residue; while some kinds of expectorated matter, of the consistence of mucilage, afford only one fortieth of dry residue; and others, of the consistence of thin paste, afford only one fourteenth of residue.

Full of globules, which seem to indicate organization.

10. But for the unavoidable extent of this paper, I should trouble the learned Society with various other conclusions and remarks, especially concerning the *globularity* of expectorated matter, which seems to indicate organization. Although Antonius Van Lewenhoeck, above a century ago, discovered the globularity of blood, and even noticed it in other animal fluids, neither he, nor any other person, as far as I know, investigated the subject in any fluid but the blood, till by Mr. Home's acuteness and industry, at a very early period of life, it was observed in pus. I have in this paper related, that expectorated matter, especially the opaque ropy kind, as well as the puriform, is full of globules, and that, except by such agents as destroy charcoal, they are scarcely destructible. Do these spherical particles consist chiefly of organized carbonaceous matter?

Are they organized carbonaceous matter?

V.

V.

Abstract of chemical Experiments on the soft Roe of Fishes; by Messrs. FOURCROY *and* VAUQUELIN*.

Method pursued by the authors.

THE memoir of Messrs. F. and V. is divided into five sections. In the first the authors give an account of several experiments made with the roes of carp, in order to ascertain their principal properties: in the 2d they examine the phenomena, that take place when they are burned in an open fire; in the 3d, the products they yield in distillation, and the nature of the coal left in the retort; in the 4th, the manner in which they comport themselves with cold and boiling water; and with alcohol: and the 5th contains a recapitulation of the heads of the preceding experiments, with some applications of them to physiology.

SECT. I. *Preliminary experiments.*

General characters of soft roe.

1. The roe is distinguished from the other organs of fishes by its soft consistence; by its texture, which is somewhat greasy and smooth to the touch; and particularly by its fishy smell.

Neither acid nor alkaline.

2. It is neither acid nor alkaline.

3. When triturated with a concentrated lixivium of potash it emits no smell of ammonia; and on the addition of a fresh quantity of alkali it forms a thick magma.

4. Thirty gram. [463 grs] of roe, dried in the air by a gentle heat, being mixed with 6 gram. of potash, and afterward diluted with water, afforded when distilled merely some traces of volatile alkali, which evidently arose from a small quantity of muriate of ammonia, that naturally exists in the roe.

Contains no ammonia.

These two experiments prove, that the roe contains no perceptible quantity of volatile alkali.

Action of heat.

5. Roe dried slowly by a gentle heat in the open air loses three fourths of its weight. It turns a little yellow, and becomes friable.

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* Annales de Chimie, vol. LXIV. p. 5.

6. Heated in a platina crucible, it first hardens, then softens, and at length melts in great part. Yellow fumes are emitted, which have the acrid smell of animal fats.

The coal contains an acid,

7. The coal of the roe, being washed with warm water, communicates to it a *very decided acidity.* This solution gives a precipitate with alkalis: if it be evaporated to dryness, and the residuum treated with water, a white substance separates, consisting of the phosphates of lime and magnesia: if ammonia be added to it, ammoniaco-magnesian phosphate and phosphate of ammonia are obtained.

which is the phosphoric,

Water therefore extracts from the coal of the roe *free phosphoric acid,* and a little *phosphate of lime and of magnesia.*

The phosphoric acid obtained certainly did not exist in the roe, for this is not acid. On the other hand the roe contains no volatile alkali, that can be made evident to the senses. Now as the phosphate of ammonia is the only salt, that can yield phosphoric acid by the means of heat alone, we must conclude, *that this acid is formed during the combustion of the roe,* which is a very remarkable fact, and altogether new.

and formed during the combustion.

Roe calcined

SECT. II. *Combustion and calcination of the roe in an open fire.*

Perforated the platina crucible.

1. One hundred and seventy-eight gram. [2748 grs.] of fresh soft roe yielded 7·8 gram. [120 grs.] of coal. This coal, strongly calcined in a platina crucible, grew red, and perforated the crucible. The metal was rendered brittle.

Yielded phosphate of lime,

2. The aqueous lixivium of the coal was acid. On evaporation it let fall 45 centig. [7 grs.] of phosphate of lime. The lixiviated coal weighed only 5 gram. [77 grs.]

phosphoric acid,

3. The lixivium deprived of phosphate of lime, and saturated with volatile alkali, yielded 5 gram. [77 grs.] of dry phosphate of ammonia.

and phosphate of magnesia.

4. This phosphate of ammonia contained a little phosphate of magnesia; for, being heated before the blowpipe, it yielded a transparent pearl, which became opake on cooling, and was not wholly soluble in water.

Phosphorus formed from it.

5. On distilling 5·6 gram. [86·5 grs.] of this phosphate of ammonia with 1 gram. [15·4 grs.] of charred cork in a

coated glass retort, 26 cent. [4 grs.] of phosphorus were produced. The residuum of the distillation weighed 4·2 gram. [65 grs.], and still contained a great deal of phosphoric acid. The muriatic acid, with which it was washed, yielded, on the addition of limewater, 4·5 gram. [69·5 grs.] of phosphate. It contained a little phosphate of magnesia.

Separation of the phosphates of lime and magnesia.

6. Messrs. F. and V. always remarked, that the coal of the roe calcined some time, and afterward once lixiviated with water, yielded an acid liquor containing a little lime, and a great deal of magnesia. These two phosphates are easily separated by evaporation to dryness, and applying water to the residuum, when the phosphate of magnesia will dissolve, and that of lime be left behind. Ammonia separates from this solution but a very small quantity, because it forms a soluble triple salt with phosphate of ammonia.

SECT. III. *Distillation of the roe, and examination of its coal.*

Roe distilled.

1. The apparatus employed for this purpose consisted of a well tried stone retort, communicating, by means of an adopter, with a glass receiver, from the tubulure of which issued a curved tube terminating in a phial full of oximuriatic acid. This was for the purpose of knowing whether there were any phosphorus in the gasses evolved. The fire was managed with a great deal of caution, and raised gradually till the bottom of the retort was at a white heat.

Products.

123 gram. [1899 grs.] of fresh roe yielded the following products:

a, A great deal of colourless water;

b, Some white, or slightly lemoncoloured, oil;

c, An oil as red as blood, and tolerably fluid;

d, A blackish brown thick oil;

e, At the same time with this oil some salts, which condensed in needly crystals on the sides of the adopter;

f, A white crust, mottled with yellow and red, adhering to the upper part of the adopter;

g, There was but little carbonic acid and carburetted hidrogen gasses evolved.

2. We shall now examine the nature of these products.

The

Prussiate of ammonia.

The water (*a*) contained some carbonate, a great deal of prussiate, and some traces of muriate of ammonia.

Phosphorus.

The crust (*f*) was *pure phosphorus*, for it smoked on the contact of air, diffused the smell of phosphorus, was luminous in the dark, and inflamed rapidly on increasing its temperature.

The Oils examined.

The oils (*b*, *c*), heated with nitric acid at 30°, emitted white fumes having a smell of phosphorus mingled with that of oil. The capsule in which this operation had been performed, was luminous in the dark. In order to collect the phosphorus, the liquor was poured into a glass retort, and the distillation urged, till the greater part of the oil was destroyed by the action of the nitric acid. Some water passed over into the receiver, containing carbonic, prussic, and muriatic acid. A light oil likewise came over. These products contained no phosphorus. The oil that remained in the retort was partly converted into a red bitter matter, partly into a kind of wax, which congealed on the surface of the liquor in cooling.

The nitric solution, evaporated to the consistence of a sirup, afforded crystals of nitrate of ammonia. The mother water contained phosphoric acid, proceeding from the phosphorus acidified by the nitric acid.

The coal.

The coal left after the distillation of the roe weighed 7·5 gram. [116 grs.]. It was not acid; did not become so by calcination; and did not inflame during this operation.

Phosphorus an elementary part of roe.

This experiment shows, 1, that the coal of distilled roe is not phosphuretted, like that of roe calcined by a gentle fire in the open air: 2, that phosphorus is an *essential element of roe;* a fact that has never been mentioned by any one, nor has its existence as an element in any animal substance ever been suspected.

With less heat the coal a phosphuretted carburet, and scratches glass.

3. When the heat is not carried so far as in the preceding distillation, no phosphorus is obtained, it remaining combined with the coal in the state of a phosphuretted carburet. This coal is the hardest afforded by any organized substance, for it scratches glass. When heated gently in a platina crucible, a yellowish green flame is produced; and when the temperature is raised to a dull red, this flame becomes in-

termittent. After this calcination the phosphorus is acidified, and may be dissolved in water.

The phosphorus in the coal not owing to the decomposition of any phosphate.

Desirous of removing any doubt, that might arise respecting the state of the phosphorus in the coal of roe, the authors lixiviated this coal with muriatic acid. This dissolved the phosphates of lime and magnesia; and the residuum, calcined afresh, exhibited the same phenomena as it did before it was thus treated. The phosphorus of the coal of roe, therefore, is not owing to the decomposition of these phosphates.

Carbon has a strong affinity for phosphorus.

The affinity of carbon for phosphorus appears to be very great; for the same coal, being calcined four times, yielded phosphoric acid each time.

Phosphate of soda and of potash, and nitrogen.

Beside the phosphates of lime and magnesia, which the coal of roe retains, we find also phosphate of soda and of potash, which may be dissolved in water. It contains nitrogen too; for when it is calcined with potash we obtain prussiate.

Coal of fibrin.

4. The coal of fibrin, subjected to the same experiments as that of roe, does not exhibit the same phenomena. The water with which this coal was washed was perceptibly alkaline; which shows, that the acidifiable property observed in the coal of calcined roe does not belong to all animal coal; and there is even reason to believe, that it is peculiar to this substance.

SECT. IV. *Roe treated with water and with alcohol.*

Roe treated with water.

1. Roe triturated with distilled water, and reduced to a pulp, imported to it no signs of acid or alkali. Its soft and white part is diffusible in water, and gives it the appearance of an emulsion; the membraneous matter however is perceptible in it, and cannot be separated; and when filtered, it is still turbid.

Contains albumen and gelatine.

2. If we boil the water in which roe has been diffused, there is one portion which coagulates like albumen; but the water retains in solution a matter analogous to gelatine, for it becomes a jelly by boiling down.

Jelly.

3. This jelly, being burned and calcined, exhibited no phos-

phorescence, or phosphoric acid; only sulphuric acid extracted from its coal phosphate of lime and of magnesia.

Soluble portion.

4. The soluble part of roe is precipitable by nutgalls and several metallic solutions.

No phosphate of ammonia.

5. Roe being boiled a long time in water, not an atom of phosphate of ammonia was extracted;, which confirms what has been said above of the existence of phosphorus in a pure state in this animal substance.

Insoluble part.

6. The portion of roe not soluble in hot water being carbonized, and then lixiviated with water, it yielded no phosphate of ammonia. It afterward comported itself as the coal of the entire roe. The phosphorus, therefore, remains with the albumen.

Roe treated with alcohol.

7. Alcohol takes up from roe a kind of saponaceous matter, which imparts to it an unpleasant smell and taste.

When this alcohol is distilled, the liquor becomes of a greenish yellow; and, when all the fluid has passed over, a substance remains resembling soap both in taste and smell. This soap retains some atoms of alkaline phosphate.

The roe treated with alcohol has no longer a greasy feel, but is dry and harsh. It appears therefore, that its unctuosity is owing to the animal soap.

SECT. V. *Conclusion.*

Discovery of pure phosphorus in organized substances.

1. The discovery of phosphorus in the combustible state in organized substances belongs entirely to Messrs. Fourcroy and Vauquelin*; for, if we consult those chemists who have attempted the analysis of compounds of this kind, we find only Margraff, who says he obtained phosphorus in distilling a vegetable substance. Mustard seed, with which he made his experiment, being subjected to a fresh examination, did not furnish the slightest trace of this combustible. Hence it is probable, that the phosphorus obtained by this chemist was an accidental product of the operation.

Mustard seed.

Proposed inquiry.

2. The authors of this paper purpose to inquire, whether this phosphuretted animal compound belong to the organization of fishes in general, or be peculiar to their soft roe.

Phosphores-

They presume, with much reason, that the phosphorus found in an organ so essential has some influence in the phosphorescence of fish; and that hereafter perhaps we may discover this singular property to be owing to the same cause, not only in several marine animals, but also in several insects that dwell upon land.

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* This is a mistake. See the following article. C.

cence of fish and other animals.

VI.

On the Phosphorus, that Seeds afford by Distillation; and on the Decomposition of Alkaline Phosphates by Charcoal:

by THEODORE DE SAUSSURE[*].

SECT. 1. THE interesting inquiries of Messrs. Fourcroy and Vanquelin on the roe of fishes published in the Annales de Chimie[†], and their discovery of phosphorus in the combustible state among the products of the distillation of this animal substance, led to the observations that form the subject of this paper.

Phosphorus in animal

In the account of their labours it is said, that phosphorus had never before been supposed to be found in any organized substance, except by Margraff in mustard seed; and this they conceive to have been accidental, as their examination of this seed gave no trace of it[‡].

and vegetable substances.

The silence of modern writers on this discovery of Margraff seems to render it questionable; since the observation appears too singular and striking, for no one to have attempted to confirm it and investigate its cause. That chemist however does not claim for himself the discovery of phosphorus in the distillation of seeds. He first speaks of Albinus and Hoffmann, who had before obtained phosphorus by distilling the seeds of mustard, rue, and rocket, with a very vehement fire. He afterward mentions Pott, who had taught him, that wheat, rye, and other grain, yielded phosphorus. And lastly he relates his own results, which confirm those of his predecessors. Such testimony could have left no doubt of the reality of the observation; if

Margraff not the only chemist that found phosphorus in seeds.

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[*] Annales de Chimie, vol LXV, p. 189.

[†] Vol. LXIV, p. 5. See the preceding article.

[‡] See p. 278.

if the considerations I have adduced above had not led modern chemists to adopt a contrary opinion.

Beans appear to contain phosphorus.

Several years ago, as I was incinerating some beans in a platina capsule, I exposed them suddenly to too strong a heat. The charcoal and ashes of these seeds agglutinated together at the bottom of the capsule, and a hole was made through the metal in one place where it was in contact with the charcoal. I did not then ascribe the effect to its true cause; but when I found Messrs. Fourcroy and Vauquelin had experienced a similar incident when treating fishes roes in a platina vessel, I concluded, that the observations of Pott, Margraff, &c., were well founded. As the success of the process depends in great measure on its minutiæ, I shall relate all the particulars of that I adopted.

Wheat distilled.

I subjected to distillation in a stone retort 1·039 kil. [16042 grs.] of wheat. The fire was at first very gentle, and raised by degrees, in the course of three hours, to a slight white heat. When at this temperature no more vapour was evolved, I took out the coal that had formed in the retort, and which weighed 250 gram. [3860 grs.] This I powdered and introduced into a small coated porcelain retort, which it three parts filled, and which communicated by means of an adopter with a tubulated receiver half full of water. After having luted all the junctures of the apparatus, I placed the retort in a furnace, the fireplace of which, 2·3 dec. [9 inches] broad, terminated in a tunnel 1·3 dec. [4½ inches] in diameter, and 2 met. [6 feet 6 in.] high; and I raised the fire gradually in about two hours to as intense a heat as the furnace would produce. This heat was sufficient to fuse the argillaceous lute that coated the retort, and remove almost the whole of it. A white smoke, having the smell of phosphorus, then diffused itself through the receiver.

Phosphorus in the adopter.

When the apparatus was cold, I unluted it, and found a slight coating of phosphorus in the adopter. It took fire on the admission of air, and exhibited the smell, colour, consistency, and all the other characters, by which this eminently combustible substance is so easily distinguished.

The success depends on the management of the heat.

From this result we may presume, that other seeds would furnish the same product; and that, if it have not been obtained by all chemists, it was because they did not employ a degree of heat high enough, a sufficient quantity of seed, and all the precautions I have mentioned above. I have reason to think for instance, that, if the degree of heat requisite to disengage the phosphorus be employed at the commencement of the operation, this product will not be obtained separately, because it would combine with the hidrogen gas of the coal.

The coal of wheat, treated with boiling water before the extraction of the phosphorus, yields a lixivium that turns sirup of violets green, and is indebted for this property to potash. In this instance therefore the phosphorus appears not to be owing to the action of the charcoal on free phosphoric acid. Neither does it arise from the decomposition of phosphate of ammonia; for the heat requisite to carbonize the seed appears to be more than sufficient to volatilize this salt.

The coal of wheat does not contain free phosphoric acid or phosphate of ammonia.

As I found a considerable quantity of phosphate of potash in all the ashes of the seeds I have analysed, particularly in wheat (see my Chemical Inquiries concerning Vegetation), I thought it necessary to examine, whether this salt be decomposable by charcoal. I confess however it is not certain, that this phosphate exists already formed in the coal made in close vessels; for the water with which it was lixiviated afforded me none, or at most scarcely perceptible traces of this salt; but the coal, from its great porousness, retains substances intermingled with it so pertinaciously, that we can conclude nothing from this operation.

Considerable quantity of phosphate of potash obtained from all seeds.

SECT. 2. *Decomposition of the Phosphate of Potash by Charcoal.*

Among the neutral phosphates we know only the phosphate of ammonia, and those of some metals, that are decomposable by charcoal. All the chemists who have engaged in these inquiries have asserted, that phosphate of potash, phosphate of soda, and phosphate of lime without excess of acid, resisted this decomposition. It is on a supposition therefore, that these authors might have been mistaken, that I made the following experiment.

Phosphates decomposable by charcoal.

Those of potash, soda, and lime, supposed not.

I neutralized pure potash with phosphoric acid prepared by the action of nitric acid on phosphorus. To prevent all

Phosphate of potash decomposed by charcoal.

suspicion of excess of phosphoric acid, I added a slight excess of alkali to the phosphate of potash, so that its solution turned blue colours green. This phosphate, dried at a red heat, weighed 30 gram. [463 grs.]. It was powdered and triturated with twice its weight of beech charcoal, which had been red hot just before they were mixed. I put the whole into a small coated porcelain retort, and exposed it for four hours, in the furnace described in the preceding section, to the same degree of heat as in the preceding operation. The phosphorus flowed by drops into the water of the receiver; and, though I extinguished the fire while some phosphoric fumes were still evolved, I collected in the receiver and in the adopter, 2·5 gram. [38 grs.] of phosphorus. In this weight I do not include a portion of this substance, which adhered to the neck of the retort; what was suspended in the water of the receiver, and rendered it yellow like whey; or what was combined in the phosphuretted hidrogen gas produced in this distillation. If it be considered, that 30 gram. of phosphate of potash contain by calculation about 4·8 gram. [74 grs.] of phosphorus only, there can be no doubt, that this salt was almost wholly or in great part decomposed by the charcoal.

SECT. 3. *Decomposition of Phosphate of Soda by Charcoal.*

Phosphate of soda decomposed by charcoal.

For this experiment I employed crystallized phosphate of soda, with a slight excess of the alkali, according to the common practice of the shops for obtaining regular crystals. By drying at a red heat 100 parts of this salt were reduced to 40·94. I powdered 30 gram. of this phosphate, and mixed with it 60 gram. of beech charcoal, which I made red hot immediately before mixing them. The operation was conducted as in the preceding instance, and I collected the same product, namely 2·5 gram. of phosphorus, with the same omissions as before.

As the phosphates of potash and of soda deprived of their waters of crystallization contain nearly similar quantities of phosphoric acid, we may deduce from them the same conclusions.

Why other chemists have failed.

The decomposition of the phosphate of soda by charcoal has been a particular object of inquiry to chemists. It is to be presumed therefore, that they failed either because they did not deprive this salt of its water of crystallization, or because they did not employ a degree of heat much beyond what is necessary for obtaining phosphorus from free phosphoric acid.

SECT. 4. *Decomposition of Phosphate of Lime by Charcoal.*

Phosphate of lime decomposed by charcoal.

I mixed an aqueous solution of nitrate of lime with phosphoric acid, and precipitated the solution with ammonia, which separated from it the phosphate of lime. Of this salt, well washed, powdered, and dried at a red heat, 10 gram. [154 grs.] were mixed with twice their weight of beech charcoal powder, which had previously been boiled in a large quantity of water to deprive it of potash.

The degree of heat employed in the preceding operations was not sufficient to render the result of this very decisive. I therefore gave up all thoughts of extracting the phosphorus by distillation in the furnace above described; and put a similar mixture of charcoal and phosphate of lime into a Hessian crucible, closed with a platina cover, and surrounded with charcoal powder in another crucible closed with an earthen cover. This apparatus was exposed for two hours to the action of a wind furnace, which produces the highest degree of heat, that Wedgwood's pyrometer will indicate. The interior crucible was not altered, but its platina cover was fused. The earthen cover remained entire. The residuum of the mixture of phosphate with charcoal was digested in muriatic acid. The solution, filtered and precipitated successively with ammonia and carbonate of ammonia, yielded to the former 4·52 gram. [70 grs.] of phosphate of lime dried at a red heat, and to the second 5·22 gram. [80 grs.] of carbonate of lime. The result shows, that about half the phosphate of lime was decomposed by the charcoal.

Native phosphate more difficult to decompose.

I repeated the preceding experiment on native phosphate of lime, the transparent Spanish chrysolite in regular crystals [*]. As the elements of this substance are much more condensed than those of the artificial phosphate, its decomposition could not be expected to proceed so far. From 6·26 gram. of chrysolite, powdered and mixed with twice its weight of powdered charcoal, I obtained, by the same process as above, 1·22 gram. of carbonate of lime, and 4·95 of phosphate of lime.

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[*] Mr. Vauquelin has found, that 100 parts of chrysolite contain 53·3 of lime, and 45·7 of phosphoric acid.

Beech charcoal.

I should mention, that 20 gram. of beech charcoal powder, treated with muriatic acid, yielded only 5 thousandths of a gramme of phosphate of lime, and 6 thousandths of a gramme of carbonate.

All organized substances will yield phosphorus.

From these inquiries it appears, that there is no animal or plant, but may yield phosphorus, or at least phosphuretted hidrogen gas, by simple distillation with a very vehement fire; for we know none of those substances, that do not furnish phosphate of lime and charcoal.

Degree of heat important.

It follows too, that the quantity of the phosphates obtained by incineration may vary, according to the degree of heat employed in the operation.

VII.

A simple and secure Method of joining long or bent Metallic Pipes, for Chemical Purposes; with some Queries. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Dr. Henry's juncture for metallic pipes

IT is but lately, that I have made chemistry a part of my studies, and it is therefore with considerable diffidence, that I take the liberty of addressing you. In reading Dr. Henry's account of a portable copper boiler, for exhibiting the most important facts respecting latent caloric*, I could not help thinking, that a more simple, and at the same time more secure, method of joining together long or bent metallic pipes, might be adopted, than by means of the stuffing box described in the passage alluded to. The plan which I propose to adopt consists merely in having a small

Different one proposed.

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* See Epitome, p. 487, 5th edition.

tube made, of the same materials as the larger ones, with a screw at each end, which is to be made to screw either into or over them. These screws must be cut in different directions, so that by turning the small tube in one direction, they will at the same time screw into both the long pipes. Should one upon trial be found to screw up to the shoulder sooner than the other, it will be requisite to file a small piece off the end of the longer top, and by this means they may be made to fit accurately, and to screw up exactly at the same instant. If the screw be cut upon the lathe, it will only be necessary to move the tool from left to right, instead of from right to left, as in cutting a common screw.

At present I have only tried this in a very rough manner, but from what I have seen, I have not the least doubt but it will perfectly answer the end proposed.

Query on oxigen containing carbon, and the evolution of hidrogen from potash & iron filings.

Permit me now to ask an explanation of two passages in your excellent Journal, from which I have derived much pleasure and information, and for which I am happy to embrace this opportunity of thanking you. The one occurs in page 56 of vol. XXIII, No. 101, May 1809, and is as follows: "Oxigen is found to contain carbon." The other passage is met with in page 150 of vol. XXII, No. 97, for the same year. They are the words of Mr. Davy—"When "melted potash is slowly brought into contact with turn"ings or iron filings heated to whiteness, hidrogen gas is "evolved." Now as I cannot suppose, that water can exist in any sensible quantity in either melted potash or iron filings heated to whiteness, I am at a loss to account for the presence of hidrogen; especially when I recollect, that potash acted upon by the galvanic battery evolves oxigen. With respect to the first assertion, that "oxigen contains "carbon," I have not met with it before in any of the treatises on chemistry which I have read; but if either yourself or any of your numerous correspondents would have the goodness to explain these circumstances to a very young chemist, or to inform him where he may meet with a satisfactory explanation of them, he will esteem it a very great favour, and feel himself much obliged by their kindness.

Electrical experiment.

I have only to add one thing more. In page 312, No. 99, Electrical experiment. 99, in a paper on electrical attractions and repulsions, the author, in mentioning the experiment with the tumbler and balls, says, "the glass held between the two hands is nothing more than a Leyden phial." I must beg leave to add, that any one may convince himself of the truth of this assertion, by only using a glass jar capable of holding a pint and half, or two pints, instead of a tumbler. If, after grasping it in both his hands, and inverting it over the pointed brass rod inserted into the conductor, he removes one hand, and puts it quickly to the bottom of the jar, he will receive a shock. The first time I tried this experiment, I received so strong a shock, that I was afraid of repeating it for some time. However, I have repeated it several times this afternoon, and in the presence of a second person, who also received several shocks from it: once in particular he complained of having a strong shock through the stomach, and at the same moment I had observed a flash pass between his stomach and the middle of the glass jar. I have frequently received several small shocks, but if it discharges itself at once, the shock is so strong as to be exceedingly disagreeable. A larger jar no doubt would produce more powerful effects. If any thing I have said appears to be worthy a place in your Journal, I shall be happy to see it inserted; at all events an answer to the queries proposed will much oblige, Sir,

Your obedient humble servant,

L. O. C.

ANSWER.

Junctures for metallic tubes. The juncture recommended in the preceding letter will no doubt answer on many occasions, but my correspondent will not find it a very easy matter to make it fit with accuracy. If the middle part of the connecting tube were octagonal, it would admit of the use of a key, which should be made with a joint in the middle, to screw or unscrew it; and this would often be of advantage. The rim of Dr. Henry's stuffing box, which is certainly a preferable juncture, might likewise be made of this shape for the same purpose.

I know no authority for the assertion, that oxigen contains carbon. It was no doubt founded on some mistake.

With regard to the appearance of hidrogen in Mr. Davy's experiment, I can only refer my correspondent to what has been said by that gentleman in his different papers on the decomposition of the alkalis, particularly in vol. XX, p. 323; XXII, p. 61 & foll.; XXIV, p. 105.

VIII.

Methods of raising the Bodies of Persons who have sunk under Water, or of assisting Persons in Danger in Water. By JOHN MILLER, *Esq., of Bedford*[*].

SIR,

WITNESSING, as many others did, a boy losing his life in the river in this place, for want of an expeditious means of finding and recovering his body when sunk, I wrote to and waited on Dr. Hawes, requesting him to procure for me whatever were the proper and most approved machines for that purpose. He purchased for me two sets of the Royal Humane Society's Apparatus for Reanimation; two of Daniel's Life Preservers; one of the Society's Rope Drags; and one of Dr. Cogan's Pole Drags; pointing out this last as the most approved and most efficacious machine then in use: and he gave me a sketch of a bar drag, armed with tenter hooks, and weighted with a bar of iron, to which the rope for drawing it was to be fastened.

Boy drowned.

Apparatus for recovering drowned bodies.

As soon as I had received it, I went with a medical gentleman, who had been eye witness to the fatal catastrophe, and who assisted in searching for and taking out the boy, (consequently knew the precise spot wherein he fell, and where he was found, which was within three feet of where he fell) to try the efficacy of Dr. Cogan's drag. The boy fell from off the bridge while fishing, and so far distant from

Inconvenience attending Dr. Cogan's drag.

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[*] Trans of the Society of Arts, vol. XXVII, p. 125. The gold medal was voted to Mr. Miller for these inventions.

from the shore is to render a boat necessary for us to reach the spot. The depth of the water was ten feet, and we found a difficulty, from the buoyancy of the pole in Dr. Cogan's drag, in forcing it to the bottom; and when we did, the stones, or whatever it met with, so favoured its buoyancy, as to toss it up considerably when drawing forwards, and it required the force of both hands to keep it down so as to scrape the bottom with it. The result was, that the force the person holding the drag was obliged to use to counteract the buoyancy of its pole (added to the weight and resistance of the water against it) overpowered the means of the person who had the guidance of the boat, to move it in the direction he wished; and instead of the movement of the boat directing the drag (or rather the person holding it) the drag, by the force required to keep it at the bottom, and by the weight and resistance of the water acting on its pole, governed the boat, and was as it were an anchor to it, confining the efforts of the person standing in it and using the drag, and making the action of the drag stationary instead of progressive.

Inconvenience of the bar drag.

The extreme scope of this drag not exceeding eighteen inches, may it not require too much time to traverse the uncertain space wherein the object to be searched for lies, to insure its success? The bar drag, of which Dr. Hawes gave me the sketch, in point of extent and its consequent expedition might answer where the bottom is level, and no ridges or hollows would occur; but, as being a straight piece of wood, it can accommodate itself to no unevennesses, consequently must leave holes, the most likely repository of the object sought for, unsearched.

Spot where a body lies uncertain.

One cannot but be aware, that it will seldom happen, that the precise spot can be ascertained. Either from the current of the water, the confusion or difference of opinion among spectators, the space to be searched is generally considerable and uncertain. Adverting, therefore, to what Dr. Hawes had sent me, and to the conversation I had with him, as well as the inquiries I had made, I thought something was still wanting; and the desideratum appeared to me to be a machine that would be expeditious, because it was extensive; and secure, because by accommodating itself to the

Desideratum.

ground, however uneven, it would search holes and hillocks equally. I turned my mind, therefore, to form a machine of this description, and thinking that the buoyancy of the wood would take off from the weight of lead and iron about it, were I to extend it to ten feet in length, I determined on that length for it, and I found my expectation realised in the result. I therefore denominated it a machine or drag, easily drawn by one person, that fishes an extent of ten feet at one sweep, with the certainty of finding a body, if it lies within that space, let the ground be ever so uneven, or the water ever so deep.

Machine answering it.

Permit me, Sir, to request you to submit this account of my machine to the Society of Arts &c., accompanied with models and a drawing; and should they be honoured with the approbation of the Society, I shall feel myself much gratified, as their sanction could not fail to promote their publicity.

I have the honour to be,
Sir,
Your most obedient servant,
JOHN MILLER.

Description of Mr. Miller's Apparatus for Raising the Bodies of Persons sunk under Water. Pl. VI. Fig. 1, 2, 3, 4.

This machine consists of a round piece of deal A A, fig. 1, ten feet in length, and two inches and a half in diameter; at thirteen inches from each end of it, a square piece of deal B, twelve inches in length, and one inch and a half in diameter, made firm by a bracket, is let in and glued or nailed. To this bar four six-pointed drags, C C C C, are suspended at equal distances. These drags are weighted with two pounds of lead affixed or run on to the lower end of their shafts or stems, to steady them when in action, and to keep their points from running into the ground, which had they nothing to counteract their weight and preponderancy at top they would do. The buoyancy of the bar on the one hand, and the weight of the lead at the bottom of the drags on the other, has the effect of keeping the drags

Apparatus for raising drowned bodies described.

in

Apparatus for raising drowned bodies described.

in an upright position when at rest in the water, and in a diagonal one when pulled forward, scraping the ground, but not entering it. Each drag has a swivel at both ends of its shaft or stem. Its whole length, including swivels, is about nineteen inches. At nine inches and a half from the top, the hooks, which are three only at their base, but which are subdivided at eight inches from their ends, take their rise. They are curved, and their points when turned up again are about four inches below the level of their tops, and thirteen inches asunder; and the outside point of each subdivision is thirteen inches from its adjoining one. The extreme points are split and formed into a double hook, very sharp and pointing towards the stem.

Holes are bored through the bar A at equal distances, so as the hooks when suspended may approach each other within five inches. Through those at the end, which are larger than the others, and made close to the pieces of wood let into the bar, the principal or drawing ropes D D pass. This rope is of considerable length and strength, and goes through the top swivels of all the drags. It is then made fast by wooden wedges driven into the holes through which it passes, at such a length as will suspend the two end drags a few inches below the end of the pieces of wood let into the bar. The other two drags are suspended at the same distance from the bar by lines of an equal length coming through the holes in the bar, and tied to their top swivels. These two drags, as well as the two end ones, are made fast to the principal or drawing rope at equal distances with a piece of tar-line tied to their top swivels. And the two outside drags are kept in their proper situation by the principal rope going through a staple fixed in the pieces of wood let into the bar; and the two others are kept either from approaching or entangling with one another, or the outside ones, by bored pieces of wood, *a a*, of equal lengths, placed between each drag at the bottom, through which and their bottom swivels a rope made fast to the bottom swivels of the two outside drags passes. The drags, however tied or fastened their swivels may be, always have their own rotary motion free, consequently their points by their own gravity will always assume and retain their proper position when in action. The bar clears the way for the drags, breaking and removing weeds or what else might otherwise impede their progress and action. The drags, being suspended to the bar and separated from each other by nothing but what will give way, are undulatory in their progress as the bottom is, but will yet preserve the full extent of their sweep.

Apparatus for raising drowned bodies described.

Thus formed, the machine is ready for use, and may be drawn in this shape backwards and forwards at pleasure; but should the water wherein it is to be used be thought to contain roots of trees, or any thing likely to occasion the necessity of drawing up or releasing any one of the drags from the obstacle it has met with, then another appendage is advisable: a bar E, less in substance than the leading one, but of the same length, and which, for distinction sake I call the floating-bar. Holes are made through this bar at the same distance from each other as those in the leading bar, and ropes of equal length (either ten feet, or any other length that may be chosen) after having been tied to the bottom swivels of all the drags are to be brought through these holes, and there stopped, either by a knot or pieces of cork at their ends. By this means any particular drag may be got at, without altering the position of the others, for, as far as the flexibility of the rope in the intermediate spaces between the several drags will admit, each is free and independent of the other; and since, by means of these ropes, a parallelism is preserved from the leading bar to the floating one, the floating one of course brings into view the direction the one which is sunk is taking.

Should the current of water be strong, it would carry the floating-bar before the leading one in drawing down the stream. A rope *b*, therefore, weighted with a stone or piece of lead at its end, is requisite. This will act as a kind of anchor to it, will steady it, and keep it where it ought to be, behind the leading one. If bored pieces of deal *d d*, 15 inches long, are, after passing the ropes of the floating-bar through them, made fast by wooden wedges to those ropes, at three inches distance from the bottoms of the drags, they will by their buoyancy and tension prevent these ropes of the floating bar entangling round the points of the drags.

With

Apparatus for raising drowned bodies described.

With the floating bar attached to it, the progress of the bar cannot be instantly changed from straight forward to retrograde. For without making a sweep something circuitous, the ropes would entangle. But if a rope is fastened either to the middle of the floating-bar, or to that rope which operates as an anchor to it, by means of this the whole machine may be drawn back, and the same sweep repeated as often as required.

Should the floating-bar, for the purpose of disengaging any particular drag, be thought unnecessary, but that it is desirable to know what direction the one sunk is taking, and that the drawing backwards and forwards is an object, the floating bar, provided the holes at the ends of it are made sufficiently large, will do this by changing its direction from being a following bar into that of a leading one by this means. Detach it from the drags by untying the ropes that connect it with the bottom swivels. Pull them out of the floating-bar, and then pass the two ends of the principal or drawing rope through the holes next the end of it, and let it slip down to the leading bar; its own buoyancy will bring it to the surface of the water, and the operation of a man's pulling the drawing rope, will, by compression, keep it there.

The cost of this drag and appendages is three guineas.

Fig. 2, represents the drag in the state in which it should be preserved ready for use, or the manner in which it should be carried from place to place, to prevent any part from entangling. G G is a pole over which that part of the apparatus marked A and B is laid, and on which the cords D D folded up are hung, the drags C C C C remain suspended on the nearer side of the pole, and the floating bar E is laid within the drags with the hollow tubes *d d* below it.

Apparatus for assisting persons in danger.

So closely connected with this subject of life preservation are two other machines, which I have had made, that on the presumption that a description of them will not be unacceptable, I shall annex it.

One of them I call the reel safeguard, devised by me for the security of persons going to the assistance of a drowning person, or diving for them.

The other a missile rope, capable of being flung to a person in distress, at a considerable distance from the shore.

Rope to be flung to a person at a distance from the shore.

This missile rope, (fig. 4, plate VI,) 35 yards in length, is rendered buoyant by pieces of cork fastened to it, at intervals of three or four feet. It is made fast at one end to a wooden reel A, six inches in diameter, and sixteen in length, on which it should always be kept wound, to prevent the ropes kinking, and for being in readiness. But when used, it must be unwound, because the corks are an obstacle to its running off the reel in the throw, and it should be spread on the ground, or held in the hand, free from entanglement so as not to catch or impede the throw. The throwing end of the rope is fastened to a piece of wood B shaped like the but end of an oar, as no shape can be better devised for the purpose of throwing it from the hand. The person throwing it holds fast the reel in his other hand.

The cost of this is seven shillings and sixpence.

Apparatus for insuring the safety of a person going to assist another drowning.

The reel safeguard consists of a rope, or line thirty-five yards long, made fast at one end to a reel (A, fig. 3) six inches in diameter, and ten in length. To the other end of this rope, a brass or iron tinned ring, large enough to admit of the leather and buckle part of the shoulder-straps passing through it, is fastened. The other part consists of two straps B B, of strong sadler's web, 2 inches wide, crossing each other, and well sown together just above the pit of the stomach, and after leaving a sufficient space for admitting the arms, the ends on each side are fastened together. To the ends behind the left shoulder, a brass or iron tinned ring of an inch and half in diameter is well fastened, and to the ends behind the right shoulder, a buckle and strap 18 inches long is fastened. If the space for the arms to pass through is proper, and the ring behind the left shoulder, and strap behind the right, properly placed, the pull from behind will be so equal, that the brace will neither press on the pit or stomach, or the windpipe; two essentials in swimming and diving. To insure this safeguard being put on properly, when hurry requires the use of it, "*Over the left arm,*" "*Over the right arm,*" should be written or stamped on the inside of the web part with printer's ink. The person using this slips his arms into the brace, while another, having first passed the buckle and strap (which is at the end of the brace behind the right shoulder) through the ring at the end of the rope, puts the strap through the ring behind the left shoulder, and buckles it to the size of the wearer. This person keeps fast hold of the reel, while the wearer plunges into the water; and the facility with which the rope runs off the reel prevents its being any impediment either in swimming or diving.

When it is required to pull the person wearing it to the shore, it is not to be done by winding the rope on the reel, but by shortening the rope by passing one hand over the other as quick as possible. This will pull the wearer on his back, and from the elevated position of the person pulling, whether he is on shore or in a boat, the wearer's head and shoulders will be pulled out of the water, and the immersion of his head either from debility, his efforts, or any weight he may have hold of prevented.

The cost of this is eight shillings and sixpence.

IX.

On Respiration. By WILLIAM ALLEN, *Esq. F. R. S. and* WILLIAM HASLEDINE PEPYS, *Esq. F. R. S.*[*]

Nitrogen evolved when oxigen gas respired.

ONE of the most prominent features in our last communication[†] was the evolution of a considerable quantity of azote, when oxigen gas nearly pure was respired; and although a considerable part of this azote must undoubtedly be attributed to the residual gas in the lungs, after the most forcible attempt at expiration, yet the fact seemed to demand still farther investigation, it appearing of consequence to ascertain, whether the increase of azote was uniform throughout the latter stages of the experiment, or *solely* confined to the earlier periods.

Large propor-

By adverting to our former paper, it will be found, that in an experiment, where more than 3000 cubic inches of oxigen passed through the lungs in seven minutes and a quarter, 62 cubic inches of azote were found in the first 260 cubic inches expired, though the gas originally contained but 2·5 per cent, or only 6 cubic inches in this quantity; in the two next portions expired, consisting of 562 cubic inches, we found 56 cubic inches of azote, though this quantity of gas, before it was respired, contained only 14; these first portions were given off in about two minutes, and contained nearly 160 cubic inches of azote more than could be accounted for in the oxigen employed; hence it is plain, that a large proportion of the increase is evolved in the first periods of the process.

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[*] Philos. Trans. for 1809, p. 404.

[†] See Journal, vol. XXII, p 180.

tion of nitrogen evolved at the beginning of the process.

Our intention was particularly directed to this point in the following experiment. The oxigen, procured as usual from hyperoxigenised muriate of potash, was found to contain four per cent of azote; the experiment was conducted in the same manner as the preceding ones, except that the tubes of the gasometers were filled with oxigen, and the gas was not merely passed *once* through the lungs, but breathed backwards and forwards. In order to prolong the duration of the experiment, which began and ended with a forcible expiration, portions of respired gas were preserved for examination from each of the gasometers, in the following order:

Experiment. Oxigen respired repeatedly.

No. 1.	244	No. 5.	230	No. 9.	252
2.	294	6.	266	10.	168
3.	282	7.	254		
4.	266	8.	288		2544

The portion of oxigen remaining in the water gasometer of the original quantity, not employed in the experiment, was found upon trial to contain four per cent of azote, as before:

Summary of the Experiment.

Summary.

Bar.	Therm.	Cub. Inches of Oxigen inspired.	Cub. Inches of Gas expired.	Deficiency.	Time.
29·9	51	2668	2544	124	13 minutes;

here the deficiency was greater than we had ever remarked before; but on passing an equal quantity of common air

Deficiency very great,

from

the apparatus perfect.

from the water gasometer, and registering it in the mercurial ones, we were satisfied that the apparatus was quite perfect. It is, however, to be considered, that the respiration in this case was not natural, and that some small degree of force was required when the inspirations and the expirations were made in the mercurial gasometers, which renders this experiment rather different from those which had preceded it: and it appears to us probable, that a portion of air was forced into the extremities of the bronchiæ, which could not be suddenly expelled by the strongest attempts at expiration. Hence also, perhaps, the constant though smaller deficiency, even when the air was only once passed through the lungs; but when the process is continued for a much longer time, it is probable that the vessels recover their tone, and are able nearly to expel the whole of the volume admitted.

The air expired in the present instance, being examined in the manner described in our last paper, we found that 100 parts from each of the gasometers contained the following proportions:

State of the air after respiration.

No. 1.	10 carbonic acid	No. 4,	10 carbonic acid
	21 azote		7·75 azote
	69 ozigen		82·25 oxigen
	100		100
No. 2.	10 carbonic acid	No. 5.	10 carbonic acid
	11 azote		7 azote
	79 oxigen		83 oxigen
	100		100
No. 3.	10 carbonic acid	No. 6 to 10 mixed	10·5 carbonic acid
	8·5 azote		5·5 azote
	81·5 oxigen		84 oxigen
	100		100

We shall first calculate the total quantity of azote existing in the gas before the experiment, and afterwards estimate what was produced in the different periods during the first half of the experiment.

Calcu-

Calculation for Azote.

2668 cubic inches of oxigen were employed containing four per cent azote:

Nitrogen calculated.

100 : 4 : : 2668 : 106·72

the total quantity of azote in the gas consumed was 106·72 cubic inches.

Azote found after the Experiments.

	Cubic Inches			Azote found
No. 1.	244	100 : 21	: :	244 : 51·24
2.	294	100 : 11	: :	294 : 32·34
3.	282	100 : 8·5	: :	282 : 23·97
4.	266	100 : 7·75	: :	266 : 20·61
5.	230	100 : 7·	: :	230 : 16·10
6 to 10.	1228	100 : 5·5	: :	1228 : 67·54
			Total	211·80 cubic inches

The whole azote, found after the experiment, was ………………… 211·80 cubic inches

Azote detected by the same tests before the experiment only ………… 106·72 cubic inches

Increase of azote 105·08

Increase of nitrogen.

Now, as the whole time was thirteen minutes, if we divide this by the number of gasometers filled, it will give us one minute eighteen seconds for each, and the following will be the periods in which the azote was evolved.

	Time. ′ ″	Azote found.		Azote in the Oxigen.		Increase.
No. 1.	1 18	51·24	less	9·76	equal to	41·48
2.	1 18	32·34	—	11·76	=	20·58
3.	1 18	23·97	—	11·28	=	12·69
4.	1 18	20·61	—	10·64	=	9·97
5.	1 18	16·10	—	9·20	=	6·90
6 to 10.	6 30	67.54	—	49·12	=	18·42
	13 min.	211·80		101·76.		110·04

Here

Could this have existed in the lungs before the experiment?

Here the increase of azote appears rather greater, viz. 100 cubic inches, but the calculation in this case is made upon the gas *expired*, and from the above statement, we may see, that the evolution of azote goes on diminishing; we have sometimes even found, that towards the close of an experiment it has almost been reduced to nothing. The question now is, whether this increase of azote can be owing to the residual gas contained in the lungs at the beginning of the experiment, or whether a portion of oxigen is not actually exchanged for azote, when pure oxigen gas is respired.

or has it been exchanged for oxigen?

Here it may be useful to compare the azote found in our former experiments on oxigen, with the present.

Results of former experiments.

	Bar.	Therm	Oxigen Gas inspired.	Gas expired.	Deficiency.	Time.	Quantity respired in a Minute.	Azote evolved.	Inferred Capacity of lungs.
		°				′ ″			
No. 1.		53	3260	3193	67	9 20	348	110	141
2.	30·3	70	3420	3362	58	7 25	461	177	225
3.	30·15	70	3130	3060	70	8 45	357	187	236
4.	29·9	51	2668	2544	124	13	205	105	133

The greatest increase of azote was in the 2d and 3d experiments, when the thermometer was at 70°, which might materially influence the results: in the other cases, it was not higher than 53.

Capacity of the lungs,

From the experiments of Goodwin, we might be inclined to admit the capacity of the lungs, inferred from the 1st and 4th experiments, as very possible; but it seems difficult to conceive, that it can amount to 236 or 225 cubic inches, and yet this must be the case, unless a portion of azote is given off from the blood, or there is some process in nature by which it is capable of being produced from oxigen.

Having, by the kindness of our friend Henry Cline, jun., been furnished with the lungs of a stout man, about five feet ten inches high, taken from the body not long after death, and in a sound state, we proceeded to ascertain the quantity of air contained in this organ after the most complete expiration, as in death.

Henry

examined in a stout man after death,

Henry Cline had judiciously taken the precaution to divide the trachea just below the cricoid cartilage, before he opened the thorax; he then inserted a tube with a brass stop-cock, which he tied firmly to the trachea, and attached an empty bladder to the other end. The cock was then turned, so as to communicate with the bladder, and on opening the thorax 31½ cubic inches of air were expelled into it. The weight of the lungs was four pounds one ounce. A very large glass jar, being placed in a shallow tin vessel, was filled to the brim with water, the lungs were then completely immersed, and the water which flowed over, and was the measure of their volume, weighed six pounds two ounces: we next cut a portion of the lungs into small pieces, under a large inverted glass of water, and attempted to squeeze the air from the cells, but although several cubic inches were thus procured, we were soon convinced that it was utterly impossible to arrive at our object by these means, as no force that we could use seemed capable of expelling the air from the cellular membrane, into which it escaped from the vesicles. We therefore took portions of the lungs, which weighed 2774 grains; the mass, being put into a piece of new hair cloth, was subjected to the action of a powerful screw press, and the fluid was received into a vessel; after twice undergoing this operation, the mass weighed only 660 grains. Its specific gravity was very nearly that of water, viz. ·930, water being 1·000: the fluid procured by the press was of the specific gravity of 1·019; this would make the specific gravity of the lungs ·997, water being 1·000; hence it appears, that the substance of the lungs, and the contents of the blood-vessels together, are so near the specific gravity of water, that they may be fairly considered as the same.

Then, as the mass of the lungs was equal to 4 pounds of water, though 6·2 pounds of water were displaced by them, and as a pound of water occupies the space of 28·875 cubic inches, we have the following calculation:

lbs. oz.
6 2 water displaced by the lungs
4 1 weight of the lungs
2 1, or 59·554 cub. in. of air in the lungs, to which must
be added 31·580 the volume of the air forced into the bladder on opening the thorax.
91·134

and this gives us 91·134 cubic inches, as the air contained in the lungs of this person after death; and when we reflect, that the air must have been under compression, when the lungs were immersed in water, some force being required to keep them down, and also that not less than 7 or 8 cubic inches must be contained in the fauces &c., we cannot estimate the whole at less than 100 cubic inches.

It is farther to be noted, that these 100 cubic inches would occupy much more space in the temperature of the human body, than in the mean temperature in which the examination was made; and this difference would be nearly 8 cubic inches; the air left in the lungs, after complete expiration, would therefore be 108 cubic inches; but the mean of our experiments would make it 183. found to be 108 cub. inches.

Experiment	1.	141
	2.	225
	3.	236
	4.	133
		4)735
		183

Nitrogen evolved from the blood.

We are then almost compelled to allow, that, when pure oxigen is respired, a portion of azote is given off from the blood.

Experiment on a guinea pig.

We now resolved to perform a series of experiments upon some animal, which lived wholly upon vegetable food, and made choice of the Guinea pig as one of the most manageable.

Apparatus described.

The apparatus consisted of two large mercurial gasometers, which were made to communicate with a strong trough E, Pl. VII, in the middle of which a small mahogany table D was made fast by a screw, for the purpose of supporting the animal under the bell glass A. Two holes were made through the table for the insertion of tubes to supply and take off the air, each of them communicated with one of the mercurial gasometers; the tube B delivered gas towards the upper part of the glass A, in order to bring the supply of fresh air near the head of the animal: the opening of the tube C was placed within half an inch of the table to convey off the respired air; the gasometer connected with this tube was made to communicate with a mercurial bath G, in which portions of the respired air were preserved for examination. Quicksilver being poured into the trough E, so as to rise to a level with the top of the mahogany stand, we placed a Guinea pig upon it, with the bell-glass over him, and as its edges were immersed in quicksilver, the animal was completely confined in atmospheric air: we found that his body occupied the space of 39 cubic inches, which, deducted from the cubic contents of the glass A, left 55 cubic inches for the air confined with the pig, to which must be added 5 more for that contained in the tube C.

(To be concluded in our next.)

X.

On the Construction of Theatres, so as to render them secure against Fire. In a Letter from Mr. B. COOK.

To Mr. NICHOLSON.

SIR,

Iron recommended instead of wood,

IN some former letters you did me the favour to insert in your Journal, I have recommended the use of iron in the place of wood; in my last on that subject I pointed out its use and advantages in substituting it for wood in buildings, but more particularly in adopting it for staircases, as promising a certain escape in case of fire. What I would direct the attention to in this is, the great advantage of employing particularly in public buildings.

ploying it almost entirely in the erection of public buildings, especially theatres; and although I am not an admirer and encourager of theatric representations, but, on the contrary, think they are injurious to a state, as contaminating the morals and habits of a people, and consider

Theatres conducive to immorality.

them as the very seat and emporium of vice and immorality, yet as they are permitted, it is a desirable thing, to have them erected in such a way, that, for safety's sake, the frequenters may not be in danger from any accident or other

Two burnt down,

cause. We have seen the two national theatres destroyed entirely, and that with a rapidity that no human exertion could put a stop to. Their destruction arose no doubt from

and one rebuilt,

an unpardonable neglect, or a worse cause: one has risen like a phœnix from its ashes, more beautiful than before,

but not secured against fire.

but is it not risen with all the dangers of destruction in itself? It ought to have risen immortal; it ought, as a national establishment, to have been composed of such materials, as would mock a second dreadful devastation. A fire once commenced, would it not, in this new theatre, communicate to all its parts? would it not put at defiance the power of man to suppress it, and in a few hours would it not again be a heap of ruins? Drury Lane is still in

Others about to be erected.

this situation; and as it is in contemplation to raise it to its former splendour, and as another theatre is about to be erected also, I do hope, before they are erected, that the proprietors will carefully consider how absolutely necessary it is to compose them of such materials, as will endure for ages, and that cannot be again consumed with fire. The destruction of Covent Garden was accompanied with the loss of so many lives, that no care or expense can be too much to guard against so dreadful an accident. I mention not the loss the proprietors sustained, I take not that into the scale, for what is the loss of property, when compared

The legislature should provide for the security of public buildings.

with life? The nation ought to superintend the erection of its public buildings, especially those buildings set apart for amusement. The lives of his Majesty's subjects ought to be as carefully provided for by the legislature, in their meetings together for amusement, as it provides for them

Danger from crowds trying to escape,

from their enemies from without. I always have considered all such places as extremely dangerous, not that I suppose that a fire could begin and communicate itself round the house with such rapidity as to endanger the lives of the auditors; but what is as bad, or worse, on the first appearance or cry of fire, instantly would the audience rush from their places to the doors, and hundreds of lives would perhaps be lost; for the very idea of a fire deprives a man of that command over his reason, especially if he conceive the danger imminent, that at the first scream of fire, all would press to escape, so that numbers would be suffocated, crushed, or trodden to death. The second view I would take is, that on such immense piles of building being in a blaze, if the wind should be high, vast danger might be the consequence; and in the attempt to extinguish fires of this sort, we have a recent instance in Covent Garden, how dangerous is the employment of the firemen and assistants. I will suppose, that a theatre was constructed of iron instead of wood. If the scenery during the midst of the performance was to take fire, and the whole in a blaze, the spectators might rest quiet, it could not extend to reach them; and I do think, that, although the whole house resounded with the cry of fire, the idea would fix itself on the mind so strongly, that they were sitting on iron, that the alarm would not so much affect them, if it did at all, as to produce any mischievous consequences. The very thought of the theatre being incombustible would draw to it many persons, who, from a fear of accident, might now keep away. The security it promises to the proprietors not only from real danger, but from the alarm of danger, would, I should suppose, (especially if it can be made appear, that it would be erected as cheap, or nearly so, as of wood) induce them to adopt this plan.

and in cases of high wind.

In a theatre where iron was used for wood a fire of no consequence.

Plan for a theatre.

I will give a brief description of the mode, and although imperfect, yet all I wish is to strike the mind with the idea, and induce the proprietors to give it due consideration. I will begin with the stage—all the upright and cross supporters should be iron, cast light, fitted, and screwed or pinned together, and to make it fire proof, projecting edges might be cast on the bottom of each spar, so that when laid down to lay the floor upon, tiles or quarries made thin and light on purpose might be laid between the spars, forming a solid a solid fire proof bottom. Then the boards for the stage might be laid down and screwed to the spars; the same principle acted upon through all the rooms, and all the doors neatly made of iron, with pannels to fall into the mouldings neatly screwed in, which when painted would be as handsome as mahogany. The stairs and staircases all cast and fitted together in the way recommended in your Journal, No. 107, would be much more beautiful than it is possible to make wood, without going to a very great expense, and then not half so durable. They would be much cheaper than stone also. In fact, I would introduce little or no wood at all, except the floors, and these I would lay down fire proof.—It would then be impossible for fire to be communicated to the different parts of the house, was the vilest incendiary to gain admission under cover of the night, and fire the boards of the stage and scenery, there being none, or but little admission of air from below, owing to the fire proof bottom, it could make its way but very slowly, and then only the boards and scenery could be burnt. All communication being cut off with the adjoining rooms by the iron doors, it could not consume their contents, and the supporters of the floorings, being iron, could not conduct the flame. The front of the stage and orchestra should be iron; the orchestra in particular would be extremely handsome, cast with beautiful festoons of flowers or trophies, and painted in character. The flooring of the pit laid on arches could not be in danger, and the seats of the pit also should be iron, the supporters of the seats cast light, and the seats made in pannels from 3 to 6 feet, neatly cast to fall into the mouldings with round edges, so that when the supporters were fixed, the framing laid down, and screwed firm, then the pannels should be let into the mouldings, and fastened with screws also. The seats made in this way would be quite as neat, and all the objection I see is, that iron would be colder than wood; but when they are covered, that objection in part would vanish—at all events, the supporters of the seats might be iron, if the seats were wood. The framing for the boxes might all be of iron, and the seats iron also, for they are stuffed, therefore iron could be no objection. Then the partitions of the boxes might be very

Little or no wood, but the boards to form the floors, and these fireproof.

very neatly managed and light, being all cast in open work and the fronts of the boxes might also be cast in scrolls, Gothic, or in trophies, in fact, in any way, figure, or shape, fancy might invent, more tastefully, more light and elegant, than it is possible to do it in any other way. Then, if it was wished, the fronts of the boxes might be rendered warm, if the open work was objected to as cold, by lining them inside with cloth or velvet, which would form a very handsome ground, on which the scroll or fancy work of the boxes would be seen to great advantage; the back of the boxes and doors I would propose of iron laid in pannels, and tastefully painted or lined—a person would not be able to discover, whether they were made with wood or iron inside, if well managed in the padding. The lobbies, the small staircases, the railing, the supporters and framing of all kinds should be iron, and all the floors should be laid on fire proof floorings. The grand staircase might be made to have a beautiful effect, if a man of fancy and genius was to design it; and the whole of the grand entrance might be such, as would strike the beholder with amazement at its novelty, and the mind at the beauty and delicacy of its composition, as in iron the finest specimens of antiquity could be introduced. The supports of the galleries should be iron. The gallery floor laid down fire proof, and the seats, if not iron, should be on iron supporters—the gallery stairs and staircase all iron—and the roof should have no wood at all in it, but be composed of hollow or solid iron, cast light, and if each piece was graduated from bottom to top, would still make it more light, and yet be equally as strong. A roof of this description well secured together with screws and bolts, &c., would not be considerably heavier than wood, as the iron would possess strength equal to wood, at less than half the thickness.

Comparison of the advantages and disadvantages of iron & wood.

Now it would be well to compare the advantages iron promises above wood, before it is adopted; and in looking at its advantages, all its possible disadvantages should be coupled with them, in order to see it in its true point of view. One objection might be urged, that it would be difficult to erect a theatre or great public building on this plan. But I say there would be no difficulty in procuring men competent to undertake the erection of them, and who are able to plan, get made, and put together, all its several parts; and in those parts where there was a field open for genius and taste to shew itself, in the entrances, boxes, and other conspicuous parts of the house, the most beautiful and unique ornaments might be made, and finely cast in iron, and afterwards touched up with the tool, and painted to imitate whatever fancy might dictate. There would be, especially in a theatre, the greatest scope for genius; thus might be constructed the most elegant one in the world; and one that no accident, no misfortune, no incendiary could destroy—that would brave the utmost efforts of time to destroy it—that would endure for ages. Another disadvantage might be supposed, and that is the additional expense in the erection. This might be something more, especially if beauty was suffered to lead, and genius permitted to exercise itself in the ornamental part; but suppose it was to cost ten thousand pounds more, what is it, in a public national building? especially when you are certain in constructing it in this way, you are constructing a work that will endure for ages. If you use wood, how can you assure yourselves, but by some unforeseen accident it may meet with the fate of the other theatres? Money must not be put in the scale as a competent balance against security. After considering the subject, and examining the way Covent Garden is built and fitted up, I am more and more convinced in my mind, that iron might be substituted for all the timbers, and for fitting up a theatre complete in the way I have before hinted, as cheap, or nearly so, as though it was done with wood. It is then self evident, it would be more durable, perfectly safe from fire, and much more elegant, if raised under the auspices of a man of genius and good taste.

Expense.

I am, Sir,
Your obedient servant,
B. COOK.

Caroline Street,
Feb. 20, 1810.

REMARKS.

REMARKS.

Iron recommended by others.

That the construction of theatres is a matter of public concern has already been very justly observed by Mr. Edgeworth, in his paper on this subject, in vol. XXIII, p. 129; where too the use of iron and incombustible materials is strongly inculcated. Sir George Cayley likewise, in his paper, vol. XXII, p. 241, recommends the use of iron, but not to the same extent as Mr. Cook; though certainly the more of it can be employed the better. There is one thing however of which Mr. Cook does not seem to be aware, and that is the necessity for trap doors and openings in the floor that forms the stage: but this is of little consequence, for they might be contrived in a floor of iron, as well as in a floor of wood. To obviate the objection of increased expense, Mr. Cook brings against it the increase of duration, with the probability of larger audiences. But if we likewise take into consideration the saving of insurance, it would probably be found on calculation, that the use of iron would be by much the most economical. This saving, which does not appear to have been adverted to, probably from its amount not being generally known, will be considered as of no small importance, when it is understood, that the offices were paid forty shillings for every hundred pounds insured, previous to the burning down of Drury Lane theatre; and that, since this event, they will not insure at a less premium than four guineas per cent. Now the proprietors of the theatre lately erected at Covent Garden state the expense of erecting it at £150000; the insurance of which against fire would amount to no less than £6300 a year. If, as I suppose, scenery and dresses be not included in this estimate, the insurance would be still more to cover the whole. And to insure only one third of this, or £50000, as on the old theatre, the premium would be £2100 a year. Would not the saving such a sum annually more than repay the additional expenditure for rendering the building proof against fire by the general use of iron? to say nothing of other advantages. C.

Great saving on the score of insurance.

XI.

XI.

Letter from Mr. PRIEUR *to Mr.* GUYTON-MORVEAU, *on the double Refraction of Crystals of Sulphate of Copper*[*].

Blue vitriol suspected from its crystallization to have a double refraction.

I Have long suspected, my dear kinsman, that sulphate of copper, from the form of its crystallization, must exhibit the phenomenon of a double refraction; but hitherto I have not been able to verify this conjecture, on account of the general want of transparency of its crystals. At length however I have succeeded in obtaining crystals sufficiently diaphanous for this observation, and found the double refraction very manifest.

This verified

Mode of examination.

I proceed in the known method, either viewing at some distance a pin between the crystal and the light, or looking at a luminous point produced by making a small hole in a card. But sometimes I render the effects much more evident, by looking at an object more or less distant through an opera-glass, between which and my eye I place the blue crystal: or, which appears to me in general the best method, I stand with my back to the window, and look at a very narrow slip of white paper, or a bright wire placed on a black ground.

A line joining the two images of a point observed with the crystal of blue vitriol is always in a direction nearly perpendicular to the edges of the prismatic surface of the crystal; and consequently it is requisite to hold these edges nearly parallel to the length of the pin or line we would see double, in order to obtain the greatest effect.

The crystals must be used before their surface is tarnished with that slight efflorescence, which they always acquire after being kept a certain time.

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[*] Annales de Chimie, vol. LXV, p. 188.

XII.

XII.

Table of the Rain, that fell in various Places in the Year 1809, by the Rev. J. Blanchard, *of Nottingham; with a Meteorological Table for the same Year, by Dr.* Clarke, *of that Town.*

RAIN TABLE, by the Rev. J. Blanchard, of Nottingham.

1809.	Chichester.	London.	Chatsworth, Derbyshire.	Horncastle, Lincolnshire.	Ferriby, Kingston upon Hull.	Heath, near Wakefield, Yorkshire	Manchester.	Lancaster.	Dalton, Lancashire.	West Bridgford, Notts.	Nottingham.
January	8·44	2·91	5·22	3·50	1·57	3·98	2·67	4·66	6·58	0·77	1·80
February	4·31	1·86	3·29	2·59	2·94	2·58	1·96	3·11	4·53	——	1·69
March	0·00	0·94	0·44	0·82	0·48	0·43	0·35	0·53	1·13	0·72	0·75
April	3·95	3·46	1·70	2·10	3·05	2·11	0·96	1·59	2·20	2·30	2·15
May	1·07	0·86	1·83	1·59	0·45	2·96	3·42	3·39	3·85	——	1·80
June	2·38	1·20	2·06	2·24	3·24	2·01	2·45	3·10	4·26	2·45	2·45
July	3·45	3·58	2·00	2·87	2·38	2·28	1·79	4·00	3·45	1·44	1·44
August	3·70	2·64	4·38	4·53	5·88	4·61	3·85	6·12	7·25	3·75	4·50
September ...	3·34	2·90	4·13	3·90	3·10	4·29	4·22	4·75	5·57	2·60	3·13
October	0·60	0·22	0·28	0·75	0·56	1·41	0.61	0·87	1·66	0·35	0·31
November	1·30	1·38	1·91	1·70	1·90	2·25	2·14	3·87	2·80	1·33	1·18
December ...	5·53	3·00	2·67	1·79	2·42	2·74	4·68	5·74	7·08	1·84	1·81
Total	38·07	24·95	29·91	28·38	27·97	31·65	29·10	41·73	50·36	17·55	23·01

METEOROLOGICAL TABLE,

By Dr. CLARKE, of Nottingham.

1809.	Thermometer.				Barometer.				Weather		Winds.			
MONTH.	Maximum.	Minimum.	Medium.	Greatest Variation in 24 hours	Maximum	Minimum.	Medium.	Greatest Variation in 24 hours.	Fair.	Wet.	N. & N. E.	E. & S. E.	S. & S. W.	W. & N.W.
January ..	52	17	35·29	14	30·05	28·65	29·44	1·13	18	13	14	18	9	5
February	54	30	43·00	14	30·33	28·68	29·62	0·74	18	10	2	3	33	16
March ..	62	30	44 00	10	30·38	29·00	29·99	0·40	26	5	22	6	9	15
April....	56	28	42·76	13	30·36	28·97	29·73	0·54	13	17	16	1	14	18
May	77	38	57·61	9	30·26	29·23	29·84	0·43	24	7	10	13	19	13
June	74	45	57·71	18	30·45	29 27	29·84	0·62	21	9	11	7	15	14
July	78	46	59·64	10	30·12	29·39	29·88	0·24	17	14	24	4	10	18
August ..	76	48	60·69	8	29 97	29·23	29 64	0·45	12	19	3	8	31	8
September	72	34	50 46	12	29·87	29·05	29·46	0·63	25	5	2	5	11	17
October..	67	30	52·00	10	30·25	29·77	30·09	0·38	25	6	13	6	17	4
November	64	26	42·10	11	30·41	29·03	29·89	0·77	18	12	12	2	6	22
December	53	33	40·12	13	30·00	28·25	29·45	0·90	17	14	1	6	23	15
Total..									234	131	130	79	197	165

ANNUAL RESULTS.

THERMOMETER.		Wind.	BAROMETER.		Wind.
Highest Observation, July 27th,	78°	S.	Highest Observ. June 25th,	30·45	N. E.
Lowest Observation, Jan. 22d,	17°	N.	Lowest Observ. Dec 17th,	28·25	W.
Greatest Variation in 24 hours, June 1st-2d................	18°		Greatest Variation in 24 hours, Jan. 30th-31st ..	1·13	
Annual Mean................	48·78		Annual Mean............	29·74	

WEATHER.	DAYS.	WINDS.	TIMES.	RAIN.	INCHES.
Fair	234	N. & NE	130	Greatest Quantity in Aug.	4·50
Wet	131	E. & SE	79	Smallest ditto in Oct.	0·31
		S. & SW.....	197	Total Quantity for the year	23·01
	365	W.&NW....	165		
			571		

REMARKS.

REMARKS.

On the 22d of January, the thermometer within two miles of the town stood at 14°.—April 19th, snow had fallen to the depth of one foot.—May 2d. Snow fell this morning.—Aug. 6th. The rain that fell from 9 A. M. to 5 P. M. amounted to 1·72. Loud peals of thunder at noon, increased at 4 P. M., when the lightning became extremely vivid, the thunder tremendous, and the rain descending in torrents, and continued to do so most part of the night.—Dec. 17th. The barometer at 11 P. M. stood at 28·25. The following are comparative observations on the fall of the mercury:

Barometer.

	Keswick.	Kendal.	London.	Nottingham.
Jan. 1788	28·35	28·38	28·89	——
Jan. 1789	28·09	28·12	28·58	——
Dec. 1809	——	——	——	28·25

N. B. The barometer is firmly fixed to a standard wall over a staircase, on a level of 130 feet above the sea. The pluviameter is placed in a garden, on an elevation of 140 feet above the level of the sea.

XIII.

Large Fossile Cerite, found by J. C. DELAMETHERIE*.

Large fossil murex.

I Have a gigantic fossile cerite, cerites gigas of Lamarck, which Mr. Maclure, of the Philosophical Society of Philadelphia, and I found at Grignon, on a mineralogical tour we made in the month of July. It is the largest known. Its circumference near the mouth is two or three and twenty inches [$23\frac{1}{2}$ or $24\frac{1}{2}$ E.] or about $7\frac{1}{2}$ [8] in diameter. The thickness of the lip is about 7 lines [6 E.]. Its whole length must have been about 30 inches [32], but the piece I have is broken, and not above 10 or 12 [$10\frac{1}{2}$ or $12\frac{3}{4}$] inches long.

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* Journal de Physique, vol. LXV, p. 411.

XIV.

XIV.

Reflections on the Species of Moss proposed as a substitute for Wool in stuffing Beds, Furniture, and Garments, by Mr. PARMENTIER*.

Moss proposed as a substitute for wool.

THE dearness of wool, and more particularly the property it has of imbibing putrid miasmata, and propagating contagious disorders, suggested the idea of supplying its place in beds by the *hypnum crispum L.* a kind of moss of a moderate length, and of a somewhat fragrant smell. Mr. Isengard, inspector of direct contributions at Savonne, has sent to the Society of Encouragement a specimen of this moss, taken from a mattress that has been in use for some years, with a paper in which he relates the methods of preparing it for domestic purposes.

Examined by the Society of Encouragement.

This Society, which attends to every project offered it, if its object be of public utility, and to promote our home manufactures, submitted it to the examination of its committee of economical arts; and Mr. Bouriat, one of its members, drew up a report, in which the advantages and inconveniencies, that might arise from the proposal, are justly estimated. The judicious observations in this report leave nothing to be added to them.

One of our most intelligent army physicians too, Mr. Michel, has communicated to us his ideas with respect to the hypnum crispum, both medically and economically. The following is an extract from his letter.

Bedding of the sick infectious.

"A great number of patients labouring under fevers, all of the nervous kind, in the French hospital at Genoa, where I was employed, convinced me, that the bedding of the sick is of itself sufficient, to render the slightest disorder complicated; as it is generally acknowledged, that wool imbibes contagion, and propagates it, which dangerous properties vegetable substances happily do not possess."

Moss recommended.

Among those exempt from this inconvenience he particularizes this moss, which he met with in Italy in every wood, particularly on beech trees. It is gathered in August and September,

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* Abridged from the Annales de Chimie, vol. LXV, p. 175.

September; beaten like flocks, does not retain moisture, or form into lumps like them; is little liable to decay; and costs only the price of the labour, so that four mattresses made with this moss will cost less than one of wool. It is only necessary to dry it in the shade, to preserve its fragrance.

Its advantages.

Mr. Michel observes, that neither sweat nor urine produces any fermentation in this moss, as it does in wool: but, lest moisture should occasion it to germinate, he recommends steeping it in limewater, which destroys its power of vegetation.

Not superior to wool.

The idea of employing moss for this purpose, observes Mr. Parmentier, is by no means new; for, if we may trust the poets, it was common among the ancients. It is a mistake, however, that vegetable substances cannot propagate contagion, for cotton is notoriously in bad repute on this account: and if we were to have recourse to the mosses to supply the place of wool in stuffing articles of furniture, as there are but few species adapted to the purpose, we should soon be at a loss for an adequate supply.

SCIENTIFIC NEWS.

Native muriatic acid.

Mr. von Humboldt informs us, that between Valladolid, in New Spain, and the lake of Cuisco, which is impregnated with muriate of soda, in a space of forty square leagues, there are a number of warm springs, the water of most of which contains nothing but muriatic acid, without any metallic salt.

Sea shells in gypsum.

The presence of fossil shells in gypsum is so rare, that several naturalists have questioned the fact. Messrs. Cuvier and Brongniart, in their first paper on the geographical mineralogy of the environs of Paris, say, that freshwater shells have been found in the upper strata of gypsum, and the marles immediately upon them; but they say nothing

thing of sea shells in any of the strata below the first beds of gypsum. Mr. Desmarest, of the Institute, is the only person who had mentioned turbines and other fragments of shells in the last beds of the third mass, which is the deepest; and their presence has lately been confirmed by his son and Mr. Prevost. The first fragments of these shells are discovered below what is called the great bed of gypsum. The second, consisting of more species than the former, is in a calcareous marle below what is called the little bed. All the species to which these shells belong are found at Grignon. They are calyptræ, murices, cerites, turritellæ, volutes, ampullariæ, cockles, tellinæ, citheræ, solens, corbulæ, &c. Beside these are found glossopetræ, vertebræ of fishes, claws and shells of crabs, echini of the genus spatangus, and consequently different from those of Grignon which belong to the cypeastræ. Three small beds of gypsum, and some strata of marle succeed, without any shells: and lastly we come to a bed of calcareous marle, in the midst of which is a bed of gypsum. Both these contain the same shells. They are ceritæ, which we may refer to the genera petricolum and tetebrale. In both the shell itself has disappeared; but in the first marle we find the figure of its surface in relief, and the interior filled with marle; and in the second marle and the gypsum there is a hollow moulded by the outer surface of the shell, and a nucleus moulded by the inside of the shell, while the space occupied by the shell itself is empty. Thus we have sea shells perfectly similar to those of coarse limestone, not only in marle lying between beds of gypsum, but in the gypsum itself.

Marle assuming regular figures.

Messrs. Prevost and Desmarets have made another observation on the stratum of marle, that lies below the little bed of gypsum, and which contains shells. They have found this marle in certain spots affecting the shape of quadrangular pyramids, the faces of which are striated parallel to the edges of the base. They noticed more than twenty of these pyramids, some of which are as much as 3 cent. [1·18 inch] high, with a square base of 8 cent. [3·15 inches] each way. These pyramids cannot be considered as halves of octaedra; for their base is so confounded with the marle, that the opposite faces for completing the octaedra are by no means discoverable. But they have this singularity in their arrangement, they are always found in groupes of six, with their faces touching each other, and their summits meeting in one point. The union of these pyramids produces a cube, though its sides are not perceivable, because the bases of the pyramids are continued uninterruptedly into the marle, which is precisely of the same nature as the substance of which they are formed.

Plantations of forest trees.

In the course of seven months, from october 1805 to april 1806, Mr. Curwen, of Workington hall, in Cumberland, planted on 400 acres of land upwards of 1208000 forest trees, for which he was adjudged the gold medal of the Society of Arts. He likewise planted 96100 trees in other places within the same period. Nearly half of the whole number consisted of larches.

A gold medal was likewise voted to Dr. W. Thackeray, of Chester, for having planted on the estate of his stepson and ward, between october 1804 and june 1808, on 171 acres of land, 1133759 forest and 114 fruit trees.

Plantation of oaks.

Mr. Congreve, of Aldermaston house, in Berkshire, has planted, since the beginning of 1802, upwards of 74 acres of land with oaks, for which he received the premium of the gold medal.

Map of Shropshire.

Mr. Robert Baugh, of Llanymynech, in Shropshire, has received the silver medal of the Society of Arts, and 15 guineas, as a premium for his nine-sheet map of Shropshire, published last year from an actual survey.

Double brocade weaving.

In order to confute the idea, that the silk-weavers of this country could not produce manufactures equal to the French, a society was formed some time ago, termed the Flag Association, with a view to the production of such a specimen of double brocade weaving as had never before been attempted. In consequence there is now in the loom a flag two yards wide, the ground a rich crimson satin on both sides, and brocaded on each side alike, with appropriate colours tastefully and elegantly shaded by the artist.

Upon its surface will appear woven within an oval, a female figure emblematic of the art of Weaving, reclining with pensive aspect on a remnant of brocade, lamenting the neglected state of this manufacture. A figure of Enterprise is represented in the generous act of raising her up, and reviving her drooping spirits, by showing her a cornucopia pouring forth its treasure, emblematical of the resources of this happy isle, and not unaptly indicating, that the wealth and liberality of the British nation are ever ready to support laudable undertakings, and particularly those intended for the relief of indigent merit. Close to that of Enterprise, and under a representation of the all-seeing eye of Divine Providence, the figure of Genius appears erect, pointing to a flag displaying *the Weavers' Arms*, placed upon the temple of Fame, seeming by her expressive countenance to say, "execute your arduous task, Britannia will reward your labours, and Fame, inscribing them on her sacred edifice, shall record the merits of this grand exertion to posterity." The corners of the flag will be adorned with emblems of peace, industry, and commerce. An edging with a curious Egyptian border will exhibit a combination of figures and devices, emblematic of the design for which it was formed, and the whole will in an expressive manner show to the world this interesting fact, that under the auspices of Divine Providence, and cherished by the blessings of peace and commerce, the British artists, when fostered and protected, are inferior to none throughout the globe.

The Society of Arts have presented their silver medal, set in a broad gold border, to the Flag Association.

Iron chains used instead of ropes in mining.

Mr. Gilpin has made an improvement in the pulleys in which his iron chains work (see Journal, vol. XXI, p. 111.) Finding a small degree of brightness in the links that work vertically, he made a concave rim to the pulleys on each side, to embrace the links that work flat, and prevent the others from coming into contact with the sides of the grooves. In this state the chain works 7·68 per cent easier than before; 28·87 per cent easier than a half worn, strand laid, tarred rope; and 37·51 per cent than the same chain used in the common way. Upwards of six years experience confirm his former opinion of the safety, durability, and cheapness of chains worked in grooves; yet, he observes, his men will not go down into the pit to work in the morning, or come up at night, but by a rope; though, if any scene of amusement be going forward on the surface, they will not hesitate to come up two or three together riding on the loaded basket that is drawn up by the chain.

Black pepper, in the West Indies, and cloves.

The black pepper plant thrives remarkably well in the botanic garden at St. Vincent, under the care of Dr. Alexander Anderson, and has been producing fruit there some time. The doctor finds it a plant of more easy cultivation than he had imagined. He has likewise collected a considerable quantity of cloves.

Parkinson's Introduction and Organic Remains.

Mr. Parkinson has withdrawn the *Introduction to the Knowledge of Fossils*, announced at the end of his first volume of *Organic Remains of a former World*, considering its publication as entirely superseded by Mr. Martin's excellent systematic *Outlines* of the same subject.

The *third volume* of *Organic Remains* is in considerable forwardness.

Wernerian Natural History Society.

Mineralogy of the Highlands.

At the meeting on the 3d of February, the Rev. Dr. Macknight read to the Society an account of the mineralogy of the Highlands of Scotland, from the Pass of Leny to Balahelish; which he illustrated by specimens. The general rock in this tract is mica-slate, with its usual subordinate beds, such as, of granular limestone, hornblende-slate, &c. It contains also in some districts, beds and veins of lead-glance, and indications of ironglance. Beyond Tyndrum, the mica-slate approaches to gneiss, till we pass Inverouran, where sienite appears. In the neighbourhood of King's House, newer granite, felspar, porphyry, and hornstone are found; and the adjacent country, as might be expected from the decomposition of these rocks, presents, for many miles, an unusual aspect of bleakness and sterility. Glencoe, which is singularly interesting, both in a picturesque and in a mineralogical point of view, consists of hornstone and compact felspar, in beds subordinate to the primitive rocks, and capped with porphyry. At the bottom of Glencoe, mica-slate again appears, and is covered with the formation of clay-slate clay-slate, which affords the well known roof-slate quarries of Balahelish. Thus, according to Dr. Macknight, it appears that the relative positions of the great formations, which occur in the Highlands of Scotland, correspond to the principles of the Geognosy of Werner.

Universality of rock and metalliferous formations.

At the same meeting, Professor Jameson read some observations on the universality of rock and metalliferous formations, preliminary to a short account of some specimens of a particular formation of lead ore found within fifteen miles of Dunkeld in Perthshire. The formation appeared to be almost the same with that which occurs at Strontian in Argyleshire; and it is therefore not improbable, that it may prove a source of wealth to the proprietor.

Natural history of the whale.

At this meeting, likewise, the Secretary read some new and interesting observations on the natural history of the common Greenland whale, by Mr. William Scoresby, jun., of Whitby; and exhibited a correct drawing of that animal, by the same gentleman, differing materially from the representations generally seen in books.

TO CORRESPONDENTS.

The principle in hydraulics, which my correspondent who has favoured me with a sketch of a double inverted vessel or diving bell alludes to, does not appear to be capable of producing the effect he expects from it. If I understand the description of his apparatus, the result would be, first, that air would be driven up the tube, and from beneath the upper vessel, until the lower surface of the water had risen and filled the tube; after which the escape of air would continue from the upper vessel, until the surface in both vessels were on the same level, and at this period the included air would act against the water precisely in the same manner, and with no greater advantage, than in the common diving bell.

W. N.

ERRATA.

Page,	line.		
95,	6 from bot.	The sentence beginning here, and ending line 4 from bottom, was intended to be in the form of a query: “Will not this plant perform its motions under water?”	
162,	4,	*For* 50°·40′,	*read* 50° 40′.
	11,	*After* one, *add* in respect to the convenience of the movement	
164,	10 from bot.	*For* quality,	*read* quantity.
169,	15 from bot.	— lightness,	— tightness.
170,	8 & 9 from bot.	— unanimously,	— exceedingly.
171,	9 from bot.	— lamina,	— laminæ.
187,	16,	— 4′	— 4‴.
190,	13 from bot.	— 4oz, & ½oz.	— 4 drachms, & ½ a drachm.
238,	24,	— alumine,	— aplome.

Errata in Vol. XXIV.

168,	4,	*For* complete,	*read* complicate
	8,	— *e d*	— *c d.*
	19,	— *b d e*	— *b d c.*
170,	23,	— 27·3	— 37·3
171,	13,	— 94	— 84

METEOROLOGICAL JOURNAL,

For MARCH, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

FEB. Day of	THERMOMETER.				BAROMETER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day,	Lowest in the Night.		Day.	Night.
27	50°5	48°	53°	46°	29·73	Cloudy	Cloudy
28	49·5	49	52°5	47	30·02	Fair	Ditto
MAR.							
1	50	51	53	48	29·88	Fair	Cloudy
2	50	49	52·5	37	29·70	Rain	Rain
3	40	46	52·5	38.5	29·61	Ditto	Fair
4	40	45	48	39	29·68	Ditto	Rain
5	41·5	41	44	34	29·27	Cloudy	Cloudy
6	35	41·5	42	40	28·92	Snow	Ditto
7	42·5	43	45	39	28·92	Rain	Rain
8	46	46	49	45	28·96	Ditto	Ditto
9	52	52	55	48·5	29·17	Ditto	Ditto
10	52·5	50·5	56·5	46	29·45	Fair	Fair
11	51	52	55	49	29·94	Rain	Rain
12	52	47	54	39	29·68	Ditto	Fair
13	41	41	43	37	30·03	Ditto	Ditto
14	40	36	43	33·5	30·01	Cloudy	Ditto
15	37	36	39·5	33	29·81	Fair	Cloudy
16	36	36	40	35	29·54	Hail	Ditto
17	38	34	41·5	30	29·67	Fair	Fair
18	36	35	43	31	29·88	Ditto	Ditto
19	33·5	42	44	31·5	30·02	Ditto	Ditto
20	39	40	42·5	33·5	30·05	Ditto	Ditto
21	41	46	48·5	35·5	29·76	Ditto	Ditto
22	39	37	43·5	31	29·97	Ditto	Ditto
23	36·5	42·5	48.5	36	29·88	Ditto	Ditto
24	39	40	48	34·5	29·76	Ditto	Cloudy
25	38	39	41	36·5	29·80	Ditto	Ditto
26	38	39·5	44	36	29·93	Ditto	Fair
27	45·5	45	53	41	29·76	Rain	Cloudy

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXV.

ARTICLE I.

An Inquiry, Geometrical and Arithmetical, into certain Properties of Solids in general, and of the five regular Bodies in particular. By JOHN GOUGH, *Esq.*

DEFINITIONS. 1*st.* AN edge of a solid is a line terminated by two of its solid angles, and common to two of its adjoining faces. Def. 1.

Def. 2*nd.* From *Pappus lib.* 5. Ordinate plain figures or polygons are those which are bounded by equal right lines, containing equal angles. Def. 2.

Def. 3*rd.* Two or more plain figures are said to be of the same denomination, when each and all of them are bounded by the same number of right lines. Def. 3.

AXIOMS. *Ax.* 1*st.* Three plain surfaces cannot contain a solid. Axiom 1.

Cor. Hence a pyramid under four triangles is the simplest solid bounded by planes. Corollary.

Ax. 2*nd.* Any required solid may be cut out of a given solid. Axiom 2.

PROPOSITIONS. *Prop.* 1*st.* Let *c*, *t*, *s*, be the number of the faces, solid angles, and edges of a solid, bounded by Prop. 1.

Demonstration.

polygons of the denomination n; and let p denote the number of these polygons, which bound each solid angle of the body; then we have $s=\frac{n\,c}{2}=\frac{p\,t}{2}$. For there are c faces and each face has n edges (*Def.* 3*rd.*); but each edge is common to two faces (*Def.* 1.); therefore $s=\frac{n\,c}{2}$. In like manner there are t solid angles, and each solid angle is bounded by p faces, and therefore is touched by p edges; hence $s=\frac{p\,t}{2}$. Q. E D.

Cor. 1. *Cor.* 1*st.* $n\,c=p\,t$, and $c=\frac{p\,t}{n}$; $t=\frac{n\,c}{p}$.

Cor. 2. *Cor.* 2*nd.* Now let the given solid be a pyramid under four triangles; and $n=3$; $c=4$; $p=3$; because it cannot be greater or less than 3; hence (*by Prop.*) $s=3\times4\div2=6$; and by (*Cor.* 1.) $t=3\times4\div3=4$; therefore in this case $s=c+t-2$.

Prop. 2. *Prop.* 2*nd.* If a solid angle K (Plate VIII, Fig. 6,) be contained under p planes, *viz.* A K B, B K C, C K D, D K A, &c.; and the solid angle K be cut off by any plane G H M N; the section will be a plane bounded by p right lines and p plain angles.

Demonstr. For G H M N cuts each of the planes A K B, B K C, &c.; because it cuts off the solid angle K; but its intersections with these planes, *viz.* G H, H M, M N, N G, &c., are right lines (Euclid 3.11), and any two of them are in the same plane (Euclid 2.11); therefore they are all in the same plane, and their number $=p=$ the number of planes containing the solid angle K; but the number of plane angles G, H, M, N=the number of sides G H, H M, &c.$=p$. Q. E. D.

Corol. *Cor.* If the solid angle K be cut off by a curved surface, the section will be a curved surface, bounded by p curves and p angles; and this will happen, if all or any of the faces A K B, &c., be curved.

Prop. 3. *Prop.* 3*rd.* Let A B C D K, Fig. 6, be a solid bounded by surfaces of any kind, and let c, t, s, be the numbers of its faces, solid angles, and edges; and let one of its solid angles, K, be cut off by a surface of any kind, G H M N; the increment of c together with the increment of t = the increment

Demonstr.

ment of s. For since one solid angle K has been cut off, one surface G H M N has been added to the number c (by *Prop.* 2, and its *Cor.*); therefore the increment of $c=1$; but G H M N touches p solid angles with its angles G, H, M, N, by the same, and one solid angle (K) has been cut off; therefore the increment of $t=p-1$; and the increment of $c+$ the increment of $t=p$: but no edge has been cut off; therefore the number of the edges G H, H M, M N, N G, has been added to s: but this number $=p$ also (by *Prop.* 2*nd.*) Q. E. D.

Prop. 4.

Demonstr.

Prop. 4*th.* If a solid of any kind, A B C D (Fig. 7.), have any number of its solid angles, as B, C, cut off by a surface of any kind, M K G N, so as to change the number of its faces; the increment of c together with the increment of $t=$ the increment of s. For, put $f=$ number of lines and angles bounding the additional face M K G N; $d=$ the number of solid angles, B, C, &c., cut off: then the remaining solid, A M K D G N, has one surface more than the solid A B C D; hence the increment of $c=1$; but the surface M K G N touches f new solid angles in M, K, G, N; and d solid angles have been cut away at B, C, &c.; therefore the increment of $c+$ the increment of $t=f+1-d$. Again, the additional edges of the solid A M K D G N are f in number, being the lines bounding the face M K D N; but when two solid angles are cut away, as B, C, one edge, B C, is lost; and in general when d solid angles are cut away, so as to increase the number of faces belonging to the new solid, A M K D G N, $d-1$ edges are lost; hence the increment of $s=f+1-d$. Q. E. D.

Prop. 5.

Demonstr.

Prop. 5*th.* Let c, t, s, denote the number of faces, solid angles, and edges of any solid whatever, and we have the following general expression; $c+t-2=s$. For let the solid A M K D G N be cut by one operation from the tetraedron A B C D (Fig. 7), or the solid A B C D G H M N (Fig. 6) be formed by repeated operations from a similar figure; both of which suppositions are possible (by *Ax.* 2*nd*): then the sum of the additions made to c and t by one or several operations is equal to the addition or additions made to s in the same manner; therefore the difference of $c+t$ and s in the tetraedron is equal to the same difference in the

the solid A B C D G H M N, or any other solid; that is $c+t-2=s$, (by *Cor.* 2, *Prop.* 1.) Q. E. D.

Prop. 6. ***Prop.* 6th. Let A B C D (Fig. 6) be a solid bounded by polygons of the same denomination; put $n=$ the number of sides of each polygon; $p=$ the number of polygons which touch each solid angle: then $c=4p\div 2p+2n-np$;**

Demonstr. **$t=4n\div 2p+2n-np$; $s=2np\div 2p+2n-np$. For $s=nc\div 2=pt\div 2$ (*Prop.* 1st.) $=c+t-2$ (*Prop.* 5th.); and $t=nc\div pc=pt\div n$ (*Cor.* 1, *Prop.* 1.); therefore $nc\div 2=c+nc\div p-2$; hence $c=4p\div 2p+2n-np$: again $pt\div 2=pt\div n\times t-2$; and $t=4n\div 2p\times 2n-np$; again $pt\div 2=pt\div n+t-2$; and $t=4n\div 2p+2n-np$; but $s=c+t-2=2np\div 2p+2n-np$. Q. E. D.**

Corol. 1. ***Cor.* 1st. p, which is constant relative to any given solid, denotes the number of polygons, which surround each solid angle of that body; therefore the plain angles of these polygons are equal among themselves; that is, the polygons are ordinate figures (by *Def.* 2nd.), and the preceding proposition relates to the regular bodies only.**

Corol. 2. ***Cor.* 2nd. If there be two regular bodies, one of which is bounded by ordinate polygons of the denomination n, each solid angle of it being contained under p planes, and the** other is bounded by polygons of the denomination p; each solid angle of it being contained under n planes; c in the former $=t$ in the latter; and c in the latter $=t$ in the former; but s is the same in both. This is evident from the equations in the proposition.

Corol. 3. *Cor.* 3rd. $2p+2n-np$, the common divisor of the three preceding equations, is equal to one, or greater than one; but n is at least $=3$ (Euclid, Ax. 10, Bk. 1); also p is at least $=3$ (Def. 9, Bk. 11). Now to find the limit of n, put $2n+2p-np=0$; and $p=2+4\div n-2=a, w, n$, which gives $n=6$; therefore n can only be, 3, 4, or 5; that is, the regular bodies are bounded by triangles, squares, and pentagons only.

Corol 4. *Cor.* 4th. Put $n=3$ (and by prop.) $t=12$ $6p$; hence $p=3$, 4, or 5; put $p=3$; and $c=4$; $t=4$; $s=6$, the properties of a tetraedron; put $n=4$; and $t=16\div 8-2p$; $p=3$; $c=6$; $t=8$; now put $n=3$, $p=4$; and $c=8$; $t=6$; the former of these is the cube; and the latter the octaedron; and $s=12$ in both cases (by Cor. 2.) Put $n=5$, and $t = 20 \div 10 - 3p$; hence $p = 3$; $c = 12$; $t = 20$; put $p = 3$; $n = 5$; then $c = 20$; $t = 12$ by *Cor.* 2; the former of which is the dodecaedron, the latter the icosaedron; and $s = 30$ in both (by Cor. 2.)

Prop. 7.

Prop. 7th. The sum of the plane angles bounding the solid angles of a regular body $= 8np - 16p \div 2p + 2n - np$.

Demonstr.

For the sum of the plane angles belonging to each face of the body $= 2n - 4$ (Euclid 32.1); but there are c faces; therefore the whole sum $= 2n - 4 \times c$; and $c = 4p \div 2p + 2n - np$; therefore &c. Q. E. D.

II.

Invention of a Homograph, or Method of Communication by Signals, on Sea or Land. By LIEUTENANT JAMES SPRATT, *of the Royal Navy*[*].

SIR,

Anecdote of the battle of Trafalgar.

WITH this you will receive a truly ingenious invention of Lieut James Spratt, of the Royal Navy. This gallant officer, in the glorious action of the combined fleet at Trafalgar, on the 21st of October, 1805, was on board his Majesty's ship Defiance. When engaged within pistol shot with a French eighty-gun ship called l'Aigle, he plunged into the sea, swam to the enemy's stern, and entered the gun-room port alone, made his way courageously through the different decks, and succeeded in mounting the enemy's poop, where placing his hat on the point of his cutlass, he called out to his men to join him. In attempting to haul down the French colours, he was attacked by several of their grenadiers, whom he repulsed with success. He was soon followed by several of our jolly tars, and in the act of saving the life of a French officer who cried out for quarter, a musket was levelled by a Frenchman at his own breast, which he fortunately struck downwards, but his leg was fractured by the shot; he afterwards fought two of the enemy on his knees, who were quickly dispatched by his companions, and the French ship soon after struck. More particulars of this transaction are recorded in the XV volume of the Naval Chronicle, page 193. I have the pleasure to add, that Lieut. Spratt, after a tedious illness, has recovered the use of his leg, and now has the command at the signal post at Teignmouth, anxiously wishing to be again employed in more active service against the enemies of his country.

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[*] Transact. of the Society of Arts, vol. xxvii, p 163. The silver medal was voted to Lieut. Spratt for this invention.

I have taken the liberty of sending this communication, and the account of his invention, unknown to him, knowing that the Society of Arts &c. is generally disposed to encourage merit in every rank and situation wherever found.

M. S.

Description of a simple and ready telegraph.

This new, easy, and useful code of signals is to be performed with a white pocket-handkerchief, to be held in different positions with the body. Plate VIII, figure 1, A, with the dotted lines, exhibits the whole of the numeral homograph signals at one view, (see the positions that the handkerchief is held in, and the figures marked). The first position from the right foot to the right hand is No. 1, the others No. 2, 3, 4, 5, 6, 7, 8, 9, and 0, follow in succession. When making 1, 5, 9, and 0, the handkerchief should be held by the diagonal corners, as generally prepared for wearing round the neck.

For making 2, 3, 4, 6, 7, 8, the opposite sides of the handkerchief should be gathered in each hand, the near extremity of the handkerchief to be held by one hand to the point of the shoulder.

In working the homograph the body should be erect, the positions steady, the handkerchief to be held well in front of the arms, and facing the person to whom you are to impart your intentions.

Best situation for using it in a ship,

or on shore.

The best place for showing signals from a ship is in the chains, or on a lower deck port, as the white handkerchief exhibits a greater contrast with the black sides, and is of course better discerned. When on shore, they should be made from the side of a green hill, or in front of some thick foliage, or hedge, or dark wall.

The

Method of using it.

The positions intended to compose the number of signals should be made in succession. The person, to whom a signal is made, should wave his handkerchief horizontally, to convince you that it is understood.

When the positions which compose the number of your signals are finished, you are to wave your handkerchief in like manner. For example, persons who make use of the homograph should arrange in their separate books, or from a telegraphic dictionary, every question and answer, which may occur to them on any subject, as there is no limitation to the numbers. If the number affixed to your communication be 1000, you are first to make position No. 1, and keep it so until your consort answers it by waving his handkerchief, which informs you that it is understood; then you are to make the 0 three times distinctly, as shown at figure A, each 0 to be kept up until answered as before. Now your signal being made, wave your handkerchief, which informs your consort, that he is to refer to his book for the purport of the signal No. 1000, which may either refer to a distinct word in dictionaries numbered alike, or to a whole sentence in conversations, premeditated and inserted in books formed for the purpose.

When you wish to commence a communication by signals, you are to display the handkerchief in the manner shown at fig. B, which is called the signal of attention, and your consort is to display his in return. The person who displays first has a right to begin the communication, and to prevent confusion, it is to be displayed at the commencement of every signal.

If by any accident your attention should be called off, and you did not comprehend the whole of a signal, by holding the handkerchief as in fig. C, you may demand a repetition. This signal is called the repeat. Fig. D, shows the following signals, by twisting the handkerchief regularly round one of the arms, and holding it in one of the positions marked 1, 2, 3, 4, viz. No. 1, affirmative; No. 2, negative; No. 3, interrogative; No. 4 to annul.

SIR,

Date of the invention.

I ACKNOWLEDGE the receipt of your letter respecting my homograph, and beg leave to express the high sense I entertain of its having been noticed by so distinguished an institution as that of the Society of Arts, &c. I am positively the first inventor of it, and I put it in execution at the commencement of this war, on board his Majesty's ship Defiance, commanded by P. C. Durham, who did me the honour to express his approbation of it, and promised to bring it to the notice of his Royal Highness the Duke of York; but having the honour of getting a fractured leg in the battle of Trafalgar, I was prevented from getting attention to my homograph.

Visible at the distance of 4 miles.

I have frequently conversed in this manner with my messmates at Spithead, from the green ramparts at Portsmouth, and from Plymouth Sound to the Hoe, which is still a greater distance. The conversation may be carried on at the distance of four miles by a common telescope.

Its various uses.

The various uses to which the homograph may be applied at any moment, without expense, will not fail to attract the notice of persons of discernment. In active military service it will be found very important. It will be found useful to naval captains lodging on shore, who may thus communicate any orders with ease and accuracy to the commanding officers of their ships at anchor. Passengers on board ships in fleets may keep up a constant and friendly intercourse, to console themselves for the tediousness of a long voyage; and the country gentleman may, at a moment's warning, summon his neighbours to the sports of the field, or to the hospitable board.

If my homograph should meet with the approbation of the Society of Arts &c., and tend to the good of the community at large, I shall be highly gratified.

I have the honour to be,
With respect and esteem,
Sir,
Your humble servant,
JAMES SPRATT.

Teignmouth Signal-Post,
Dec. 26th, 1808.

The

Certificates.

The following Certificates were received from Lieut. Glanville, R. N.; Lieut. Tayler, R. N.; Mrs. Spratt, of Burlington-House; and Miss Taylers, daughters of the mayor of Devizes.

I do hereby certify, that being at Ryde in the Isle of Wight in the summer of the year 1806, I frequently conversed with Lieut. Spratt, by means of the homograph invented by him; and previous to the battle of Trafalgar, Lieut. Spratt conversed in this way with many officers at Gibraltar. I also certify, that I have heard from officers of the Brest, Cadiz, and Mediterranean fleets, that Lieut. Spratt was the inventor, and the first person who made use of such invention. Given under my hand this 10th day of Jan. 1809.

J. N. TAYLER,
Lieut. of his Majesty's ship Spencer.

We the undersigned do hereby certify, that being at Ryde in the Isle of Wight in the summer of the year 1806, we did frequently see Lieut. Spratt of the Royal Navy conversing from the shore, by means of the homograph invented by him, with Lieut. J. N. Tayler, then on board his Majesty's ship the Leopard, lying at Spithead. Ann Tayler further saith, that she with many others saw it used with great success at Teignmouth.

Witness our hands, Feb. 2d, 1809.

MARY TAYLER,
ANN TAYLER,
MARGARET SPRATT.
} Devizes, Wilts.

This is to certify, that I, George Glanville, Lieut. of the Royal Navy, saw Lieut. Spratt, then master's mate of the Defiance, conversing by means of a homograph from the ship Defiance, with Lieut. Nicholas on board the Malta, six or eight months previous to the Trafalgar action, and that they seemed perfectly to understand each other by the signals given.

Witness my hand this 30th day of May, 1809,
GEORGE GLANVILLE, Lieut. R. N.

III. *Description*

III.

Description of an improved File for Receipts and Letters. By Richard White, *Esq., of Essex-Street**.

SIR,

Inconveniencies of the common file for letters and receipts.

I SEND herewith the model of a file for papers, which I think will be found preferable to any in common use. A voucher cannot be disengaged from the common file, without defacing it by cutting it off, or by removing many others to get at it; and to return it to its proper place is attended with more trouble and inconvenience. All this is avoided by the contrivance in the file now sent, the wire of which is passed through a cylinder, and fastened by a screw at the bottom.

Avoided in a new one.

I am, Sir,
Your very humble servant,
RICHARD WHITE.

Explanation of Mr. White's improved Letter File, as shown in Plate VIII, *Figs.* 2, 3, 4, 5.

This file described.

This invention consists of a metal tube *a*, fig. 3, with a convex circular plate soldered to its lower end, to keep the papers from slipping off the file, and having attached to its under side a piece of metal *b*, fig. 4, with a screwed hole in it, to receive a screw on the end of the wire *c*, fig. 5, the other end of the wire being formed into a hook, sharpened at its point, to receive the papers as usual.

Method of using it.

When any paper is wanted to be taken off the file, (instead of taking off those above it, which cannot be replaced again without much loss of time and trouble, or, which is still worse, tearing it off) the uppermost papers are to be slipped up towards the top of the wire *c*, which must be unscrewed, and, with the papers upon it, removed, as shown in fig. 5; the paper wanted may then be taken away, the wire replaced again into its tube *a*, and screwed fast, and the other papers drawn down the tube as before. The upper

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* Transact. of the Society of Arts, vol. xxvii, p. 170. The silver medal was voted to Mr. White for this invention.

per end of the tube *a* should be made conical, and its edges sharp, the better to suffer the papers to pass over it. A section of the tube and female screw *b* beneath is shown separately at fig. 4. The papers are shown in fig. 2, in the situation they are commonly placed upon the cylinder, with the wire within the cylinder.

IV.

On Respiration. By William Allen, *Esq. F.R.S., and* William Hasledine Pepys, *Esq., F.R.S.*

(Concluded from p. 301.)

First Experiment with Atmospheric Air.

Atmospheric air respired by a Guinea pig.

THE Guinea pig was placed upon the stand, and the apparatus arranged as represented in the plate: 250 cubic inches of atmospheric air were admitted into the mercurial gasometer communicating with B: the gasometer communicating with C was quite empty, the apparatus being tried was found perfectly air tight, and the whole quantity of air 310 cubic inches.

The cocks H and I being opened, gentle pressure was made upon the glass of gasometer B, so as to cause the air to pass through A, which consequently drove an equal portion through the tube C into the empty gasometer; a quarter of an hour was employed in passing the gas, which measured exactly 250 cubic inches in C, so that there was no alteration of volume; the cocks H and I were now closed, and the respired air being examined by the usual methods, 100 parts were found to contain

Results.

5 carbonic acid
16 oxigen
79 azote
———
100

As the air after the experiment had experienced no alteration of volume, and as it contained the same proportion of azote as atmospheric air, this substance had remained unaltered.

But 15·50 cubic inches of oxigen had been converted into carbonic acid gas.

100 : 5 : : 310 : 15·50.

Summary of the Experiment.

Summary.

Bar.	Therm.	Atmos. air Inspired.	Gas after experiment	Cub. inches of carb. acid.	Cub in. of carb. acid per minute	Time.
30	43°	310	310	15·5	0·62	25 min.

Experiment II. *Atmospheric Air.*

Exp. 2.

The experiment was repeated in exactly the same manner; the animal, except from confinement, appeared much at his ease all the time. The air after the experiment contained in 100 parts

5·5	carbonic acid
15·5	oxigen
79	azote
100	

Here the proportions of azote were undisturbed, and 17·05 cubic inches of carbonic acid procured.

100 : 5·5 : : 310 : 17·05

Summary of the Experiment.

Summary.

Bar.	Therm.	Atmos. air Inspired.	Air after Experiment.	Carb. acid found.	Carb. acid per minute.	Time.
29·66	38°.	310.	310	17·05	0·68	25 min.

Experiment III. *Atmospheric Air.*

Exp. 3.

The apparatus being arranged as before, we kept the pig in the glass A for one hour, and during that time passed 1060 cubic inches of atmospheric air through it, which measured 1061: portions of the respired gas had been preserved in the mercurial bath, and the usual trials made upon the mixture, which was found to contain 5 parts of carbonic acid in every 100, or 53 cubic inches in the whole quantity; the azote was unaltered; 100 : 5 : : 1060 : 53.

Summary of the Experiment.

Summary.

Barom.	Therm.	Atmos. air before expt.	Air after expt.	Increase.	Carb. acid found.	Carb. acid per minute.	Time.
29·8	56°	1060.	1061	1	53	0·88	1 hr.

Experiment

Experiment IV. *Oxigen Gas.*

Exp. 4. with oxigen gas.

The pig hitherto employed was put into the glass vessel A, which with the tube contained 60 cubic inches of atmospheric air; 250 cubic inches of oxigen, containing 5 per cent of azote, were admitted into the gasometer communicating with B, and during a quarter of an hour were made to pass slowly through the vessel, in which the animal was confined, to the empty gasometer communicating with C, where it measured exactly 250 cubic inches; a portion was preserved in the mercurial bath for examination, and the gasometer B was replenished with 250 cubic inches of the same oxigen; this was passed, in about the same time as before, through A into gasometer C, when it measured 248 cubic inches.

250 cubic inches more of the oxigen were now admitted into gasometer B, and passed in the same manner through A into C, where they measured 249.

The gasometer B was for the fourth and last time supplied with 250 cubic inches more of the oxigen, which were passed as before, through A into C, during about a quarter of an hour, and then measured 249.

Results.

The pig had remained in the vessel one hour and twelve minutes; it did not appear to have suffered in the least; portions of the respired gas were saved from each of the gasometers, and examined as usual.

	Cubic Inches.	Contained in 100 parts.		Carb. Acid.	Oxigen.	Azote.
No. 1.	250	Carb. acid	8	20		
		Oxigen	66		165	
		Azote	26			65
			100			
No. 2.	248	Carb. acid	10	24·80		
		Oxigen	78		193·44	
		Azote	12			29·76
			100			

No. 3.

	Cubic inches.	Contained in 100 parts.		Carb. Acid.	Oxigen.	Azote.
No. 3.	249	Carb. acid	10	24·90		
		Oxigen	80		199·20	
		Azote	10			24·90
			100			
No. 4.	249	Carb. acid	12	29·88		
		Oxigen	79		196·71	
		Azote	9			22·41
			100			
In Glass A, and tube C.	60	Carb. acid	12	7·20		
		Oxigen	79		47·40	
		Azote	9			5·45
	1056		100	106·78	801·75	147·52

Total, gas before experiment, 1060
after 1056
Deficiency, - 4

Calculation for Oxigen.

Calculation for oxigen.

We do not calculate upon the tube from gasometer B, because it is always in the same state after the experiment as before.

1000 cubic inches of oxigen containing 5 per cent azote, consisted of		950 pure oxigen
60 atmospheric air with the pig, and in tube C, containing 21 per cent oxigen		12·60
Total, oxigen before experiment,		962·60
Oxigen found after experiment,	801·75	
Ditto in carbonic acid -	106·78	
		908·53
Oxigen missing,		54·07

Calculation for Azote.

Calculation for nitrogen.

1000 cubic inches containing 5 per cent azote - - -	50
60 atmospheric air, containing 79 per cent	47·40
Total azote before experiment,	97·40
Ditto found after experiment,	147·52
Increase of azote,	50·12

This increase of azote was much more than equal to the cubic contents of the animal's body, the deficiency of 4 cubic inches was doubtless oxigen absorbed.

Summary of the Experiment.

Summary.

Bar.	Therm.	Oxigen, &c. inspired.	Gas after experiment.	Deficiency.
29·05	57°	1060	1056	4

Carb. acid found.	Carb. acid per min.	Time.	Oxigen missing.	Azote added.
106	1·48	1h. 12m.	54·07	50·12

Experiment V. *Oxigen.*

Exp. 5.

In this experiment we employed a smaller pig, which occupied the space of 33 cubic inches, and our object was to keep him for the same length of time in a smaller quantity of gas, we therefore only used 750 cubic inches of oxigen, beside the common air contained in the glass A and tube, amounting to 66 cubic inches. The first 250 cubic inches were passed through the glass A into C in 24 minutes, where it appeared to have undergone no alteration of volume; 250 cubic inches more were passed during the next 23 minutes, and these measured 248 in C; the last 250 were passed in 24 minutes, and the volume remained unaltered. The animal did not appear to suffer the slightest inconvenience, except from the confinement.

State of the Gas before Respiration.

State of the gas before respired.

		Oxigen.	Azote.
66 cubic inches of atmospheric air,	=	13·86	52·14
750 oxigen, containing 5 per cent azote,	=	712·50	37·50
816 total consisting of		726·36	89·64

The oxigen was tried before, as well as after the experiment, and both the results agreed perfectly with each other. We now examined portions of gas preserved from the three gasometers, with lime water, and the tests for oxigen.

State of it after.

		Time min.	Contained in 100 parts.		Carbonic acid.	Oxigen.	Azote.
No. 1.	250.	24	Carb. acid,	9·5	23·75		
			Oxigen,	60·5		151·25	
			Azote,	30			75
				100			
No. 2.	248.	23	Carb. acid,	9·5	23·56		
			Oxigen,	81		200·88	
			Azote,	9·5			23·56
				100			
No. 3.	250.	24	Carb. acid,	10	25		
			Oxigen,	82		205	
			Azote,	8			20
				100			
	66 with pig, as No. 3.				6·60	54·12	5·28
	814	71			78·91	611·25	123·84

Calculation for Oxigen.

Calculation for oxigen.

Oxigen before the experiment -		726·36
Ditto after - - -	611·25	
In carbonic acid - - -	78·91	
		690·16
Loss of oxigen		36·20

Calculation for Azote.

Calculation for nitrogen.

Azote found after experiment -	123·84
Ditto before experiment -	89·64
Increase of azote	34·20

Summary of the Experiment.

Summary.

Therm.	Gas before Exper.	Gas after Exper.	Deficiency.	Carbonic Acid found.
56°	816	814	2	78·91

Cubic Inches per Minute.	Time. h ″	Oxigen missing.	Azot added.
1·11	1 11	36·20	34·20

The quantity of azote added, of oxigen missing, and of carbonic acid formed, were smaller than in the last experiment; but the animal in this instance was smaller, as well as the quantity of oxigen passed through in a given time.

Increase of nitrogen chiefly at the beginning.

In this case, as in the human subject, the increase of azote takes place principally in the early periods. The whole azote contained in the 66 cubic inches, confined with the pig, was only 52·14; but supposing, which perhaps was not the case, that the 66 of common air were expelled by the first 250 cubic inches of oxigen, we should have

250
less 66
184 of oxigen,

containing 5 per cent azote, or 9·20 cubic inches; these added to the 52·14, would make 61·34 of azote to be found in the first gasometer, of respired gas, but we detected 75, so that even on this supposition 13·66 of azote were added in the first twenty-four minutes.

The azote contained in the second gasometer before respiration was 12·50 cubic inches, but after it had been respired for twenty-three minutes, we found 23·75, or an increase of 11·25 azote. The azote contained in the third gasometer, before respiration, was, as before, 12·50 cubic inches; but after it had been respired for twenty-four minutes, we found 20, or an increase of 7·50 azote.

The azote contained in the 66 cubic inches was 3·30, but we found 5·28, or an increase of 1·98 azote.

Blood disposed to give out nitrogen, when in deficient proportion.

From the results of these experiments it seemed, that when the *usual proportion* of azote was not present in the gas respired, there was a disposition in the blood to give out a certain quantity in exchange for an equal volume of oxigen, and we resolved to try, whether this circumstance would occur when hidrogen was substituted for azote. We accordingly made a mixture containing 22 per cent oxigen, and 78 hidrogen.

Experiment 6. *Hidrogen and Oxigen.*

Experiment 6. Oxigen mixed with hidrogen.

The pig employed in the last experiment was placed upon the stand in the glass A, with 66 cubic inches of common air as usual.

250 cubic inches of the mixture were passed from the gasometer communicating with B, through the glass A, into the gasometer communicating with C, during sixteen minutes. The animal did not appear uneasy: a second quantity of 250 cubic inches was passed in seventeen minutes and three quarters: the animal did not seem to be in the least incommoded.

A third quantity of 250 cubic inches was passed, in about sixteen minutes.

A fourth quantity of 250 cubic inches in eleven minutes and three quarters; but during this time, the animal became very sleepy, and towards the end of the experiment, kept his eyes constantly shut; he, however, appeared to suffer nothing, and was easily roused for a short time by rapping at the side of the glass. At the end of sixty one minutes and a half, he was taken out, and we found, that during this time he had produced 60·20 cubic inches of carbonic acid gas, or rather less than one cubic inch in a minute.

Less carbonic acid evolved than with oxigen.

It appears, that less carbonic acid was evolved in this instance in a given time, than when oxigen was respired, but some circumstances occurred to prevent us from discovering what change the azote had experienced: this point was, however, decided by the following experiment.

Experiment 7. *Hidrogen and Oxigen.*

Experiment 7. Oxigen with hidrogen.

Having mixed hidrogen and oxigen gases in such proportion, as that the oxigen should rather exceed the quantity contained in atmospheric air, we placed the same animal in the glass A with 66 cubic inches of atmospheric air. 250 cubic inches of the mixture were admitted into gasometer B, from the large water gasometer, and gradually passed through the glass A into gasometer C, during fifteen minutes. The pig did not appear uneasy, and the respired gas measured 250 in C: a portion of this was preserved for examination, which we shall call No. 1.

250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, during thirteen minutes; it measured 250 in C; and a portion No. 2 was preserved for examination.

The animal did not seem to suffer any inconvenience. 250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, through A into C during seventeen minutes. The animal now became quite sleepy, but did not appear to suffer any thing. He was taken out at the end of forty minutes.

Residue of the mixture examined.

At the close of the experiment, the remains of the mixture, which had stood about an hour in the large water gasometer, being examined, were found to contain 22 per cent of oxigen, and no carbonic acid; of the residual 78 parts, 20 were mixed with 10 of oxigen, which had been previously found to contain 3 per cent azote; these 30 parts being detonated in Davy's improved Volta's eudiometer, by the electric spark, were reduced to 3 parts; and these 3 parts, being treated with the tests for oxigen, were reduced to 2 parts, *a proof that all the hidrogen had been consumed;* but the 10 parts of oxigen contained 3 of azote; these deducted from 20 leave 1.7 for the azote contained in 20 parts of the residuum 78.

20 : 1.7 :: 78 : 6·6

The mixture employed, therefore, contained in every 100 parts

22	oxigen
6·6	azote
71·4	hidrogen
100	

We next examined the gas which had been respired.

Gas 1. examined.

No. 1. 250 cubic inches respired during fifteen minutes.

100 parts subjected to the action of lime water in Pepys' eudiometer, were reduced to 93·5; and these by the tests for oxigen were farther diminished to 77. 20 parts of this 77 being mixed with 10 of oxigen and detonated, the residuum,

duum, treated with the tests for oxigen, left 12 parts, which were azote.

From these 12 parts
Deduct 0·3 for the azote in the 10 parts oxigen

Leaves 11·7 for the azote contained in 20 parts of the residual 77.

20 : 11·7 :: 77 : 45

Its component parts.

No. 1 therefore consisted in 100 parts of

6·5	carbonic acid
16·5	oxigen
45	azote
32	hidrogen
100	

Gas 2 examined.

No. 2. 250, respired during thirteen minutes; 100 parts were reduced by lime water to 92·5, and these by the tests for oxigen to 77. Of these 77 parts, 20 being mixed with 10 of oxigen, and detonated, were diminished to 4; and these 4 being examined for oxigen left 3, which must be azote:

From these 3
Deduct 0·3 for azote in the 10 parts oxigen,

Leaves 2·7 for the azote contained in 20 parts of the residual 77.

29 : 2·7 :: 77 : 10·4

Its component parts.

No. 2 therefore consisted in 100 parts, of

7·5	carbonic acid,
15·5	oxigen,
10·4	azote,
66·6	hidrogen,
100	

Gas 3. Its component parts.

No. 3. 250, respired during seventeen minutes; examined as above, consisted in 100 parts, of

6	carbonic acid,
17	oxigen,
6·5	azote,
70·5	hidrogen,
100	

The 66 remaining with the animal at the close of the experiment may be considered as very nearly the same as No. 3.

Less carbonic acid evolved during sleep.

In this, as in the former experiment, we observed, that the evolution of carbonic acid was greatest at the middle of the time, but was considerably diminished toward the end, as the pig became sleepy; it is not improbable therefore, that during sleep, less carbonic acid is evolved than when the animal is exercising all its faculties.

More carbonic acid evolved than oxigen consumed.

When atmospheric air alone is respired, we have uniformly found, that the carbonic acid evolved, added to the oxigen remaining, exactly equalled the oxigen existing in the air before it was respired; but in the present instance it was one per cent more, a circumstance which we are at present unable to account for, but it was constantly the case in all the three trials.

Calculation for Azote.

Calculation for nitrogen.

From the foregoing statement we are enabled to ascertain the quantities of azote, both before and after the experiment.

Azote before the Experiment.

66 cub. inches atmospheric air, with the animal, tained $\frac{79}{100}$ or - - -	52·14
750 ———— of the mixed gasses contained $\frac{66}{1000}$ or	49·50
816 total gas employed	101.64

The total azote before the experiment was therefore 101·64 cubic inches.

Azote after the Experiment.

		Respired during		
No. 1.	250.	15 min.	100 : 45 :: 250 :	112·50
2.	250.	13 min.	100 : 10·4 :: 250 :	26
3.	250.	17 min.	100 : 6·5 :: 250 :	16·25
	66·		100 : 6·5 :: 66 :	4·29
	816	45 min.	Azote after experiment	159·04
			Ditto before	101·64
			Increase of azote	57·40

Calculation

Calculation for Hidrogen.

Calculation for hidrogen.

Hidrogen before Experiment.

The mixture before the experiment was found to contain 71·4 hidrogen.

100 : 71·4 :: 750 : 535·50

therefore the total quantity must be 535·50 cubic inches.

Hidrogen after Experiment.

No. 1.	250	100 : 32 :: 250 :	80
2.	250	100 : 66·6 :: 250 :	166·50
3.	250	100 : 70·5 :: 250 :	176·25
	66 in A	100 : 70·5 :: 66 :	46·53
Hidrogen found after experiment			469·28
Hidrogen before the experiment			535·50
Ditto after - -			469·28
Loss of hidrogen - -			66·22

More nitrogen evolved in the early than in the later periods.

In this experiment, as well as in those with oxigen, the proportion of azote evolved was greater in the early than in the later periods, and it becomes interesting to contrast them: thus we know that 52·14 cubic inches of azote were in the vessel with the animal at the beginning of the experiment, and that, of the 250 cubic inches of mixed gases passed in the first fifteen minutes, only 184 could be expelled into gasometer C, (100:6·6::184:12·14.) which contained only 12·14

making together 64·28 of azote, which was all that could have been expected in the first gasometer of 250 after respiration, supposing the *whole* of the common air had been expelled; but we detected 112·50, or an increase of 48.22 cubic inches in fifteen minutes.

The second gasometer, before it was connected with the glass A, contained 16·50 cubic inches of azote; we found however about 26, and what is remarkable, in the last gasometer there was no increase at all.

Calculation

Calculation for Carbonic Acid.

Calculation for carbonic acid.

No. 1.	250.	15 min.	100 : 6·5 :: 250 :	16·25
2.	250.	13 min.	100 : 7·5 :: 250 :	18·75
3.	250.	17 min.	100 : 6 :: 250 :	15
	66.	-	100 : 6 :: 66 :	3·96
		45		53·96

The quantity of carbonic acid evolved in 45 minutes was therefore 53·96 cubic inches, or at the rate of 1·19 cubic inches per minute.

The foregoing experiments seem to prove,

General conclusions.

1. That when atmospheric air alone is respired, even by an animal subsisting wholly upon vegetables, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas for an equal volume of oxigen.

2. That when nearly pure oxigen gas is respired, a portion of it is missing at the end of the experiment, and its place supplied by a corresponding quantity of azote; the portion evolved in a given time being greater in the early than in the later periods.

3. That the same thing takes place when an animal is made to breathe a mixture of hidrogen and oxigen, in which the former is in nearly the same proportion to the latter, as azote to oxigen in atmospheric air.

4. That an animal is capable of breathing a mixture of 78 parts hidrogen and 22 oxigen for more than an hour, without suffering any apparent inconvenience.

5. That the excitability of an animal is much diminished when he breaths any considerable proportion of hidrogen gas, or that it at least has a tendency to produce sleep.

6. That there is reason to presume an animal evolves less carbonic acid gas during its sleeping, than in its waking hours.

7. That the lungs of a middle sized man contain more than 100 cubic inches of air after death.

The facts not explicable at present.

These experiments have been conducted without reference to any particular theory, and indeed some of the results were so contrary to our preconceived opinions, that we have been induced to bestow more than ordinary attention on the subject. Confident, however, that all those who repeat the experiments with the same care will arrive at the same results, we shall rest satisfied with stating the facts, not without a hope, that those brilliant discoveries of Professor Davy, which have already given us new views of the operations of nature, will in their progress furnish us with that explanation, which it is in vain to expect at present.

Nitrogen.

Azote, or nitrogen, for instance, has been considered as a simple or elementary substance; it is recognised, however, principally by negative properties. Every gaseous fluid, which will not support life or combustion, which is not absorbed by water, or acted upon by the tests for oxigen, or capable of being detonated with oxigen gas, is generally pronounced to be azote: it is the constant residuum in almost all our experiments upon gases, but who shall say whether this residuum is a simple substance or a compound?

Is it a simple or a compound?

Is it the oxide of a metal?

The experiment of Professor Berzelius leads us to suspect it of metallic properties; and those of Davy make it probable, that it is an oxidated body; the subject is still under discussion. But we may fairly indulge more than a hope, that the ardent zeal, and well directed labours of the philosophers just mentioned, will throw a new and important light upon this obscure and difficult subject.

V.

Memoir on the Prussic Acid. By Mr. R. Porrett, *Junr., of the Tower* *.

Difference of opinion respecting the prussic acid.

CONSIDERABLE differences of opinion exist among the most celebrated chemists respecting the composition of the prussic acid, some agreeing with Fourcroy and Vauquelin, that oxigen is one of its component parts; and others with Berthollet and Proust, who dispute its presence. Mr. Proust, in his history of the Prussiates, asserts, "That there is no fact, that indicates oxigen to make a part of this acid; and that from the well-known affinities of its three elements, added to the circumstances under which it is formed, it can scarcely be thought that it does." This difference of opinion implies a want of some decisive experiments, which may set the question for ever at rest, and these which I am going to relate I am induced to think are of that description.

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* Trans. of the Society of Arts. The silver medal was voted to Mr. Porrett for this communication.

Attempt to discover an improved method of preparing triple prussiate of potash.

Some time back, I proposed to myself the discovery of a method of preparing a triple prussiate of potash, in a pure state, which should be free from the objections to which the processes in general use are subject. In reflecting on the means most likely to attain this end, it occurred to me, that I should succeed, if I decomposed prussiate of iron by double elective attraction rather than by single; employing, instead of a pure potash, this alkali in combination with a substance uniting the properties of solubility when combined with potash, strong attraction for oxide of iron, and insolubility when united to this oxide. The only substances I could think of possessing all these requisite properties were the succinic acid and sulphur. As the high price of the former precluded its use for this purpose, I determined to employ the latter. I therefore took one ounce of dry sulphuret of potash, and one ounce and a half of the best prussian blue, previously well washed and powdered, and put them into a Florence flask, two thirds filled with distilled water; a disengagement of sulphuretted hidrogen of ammonia, and of caloric, immediately took place. The materials were boiled slowly together for three hours, occasionally replacing the water which evaporated. The whole was then thrown on a filter; what remained on the filter was black, and consisted of sulphuret of iron, and undecomposed prussiate of iron. The liquid, that passed through, I found on trial to consist of triple prussiate of potash, and hidroguretted sulphuret of potash. In order to complete the decomposition of the latter, I boiled the liquid again, for the same time as before, with another half ounce of prussian blue, and when cold filtered it. The filtered liquid (A) was now nearly colourless, and free from hidroguretted sulphuret. On pouring a little of it into a solution of oxisulphate of iron, I was very much surprised to find that

Sulphuret of potash boiled with prussian blue.

The solution boiled with fresh prussian blue turned red with oxisulphate of iron, without precipitation.

that solution changed to a deep blood-red colour, without any precipitate ensuing, instead of forming with it a precipitate of blue prussiate of iron. So unexpected a phenomenon determined me to undertake an examination of this liquid; with this view I subjected it to the action of the chemical agents mentioned in the following table.

TABLE I. with Liquid A.

Action of different tests on this solution.

CHEMICAL AGENTS.	EFFECTS.
Paper stained with turmeric	No change of colour.
Paper stained with litmus	Do.
Potash - -	No disengagement of ammonia, or any apparent change.
Lime - - -	Do.
Diluted sulphuric acid	An expulsion of sulphurous acid, the liquid becomes slightly opalescent.
Nitric acid (pure)	The acid assumes a red colour, but this effect is not permanent.
Oximuriatic acid -	This acid loses its smell.
Muriatic acid (pure)	No change.
Muriate of barytes	A white precipitate.
Tincture of galls -	No change.
Nitro-muriate of platina	A heavy, brilliant, ochre-yellow precipitate.
Muriate of gold -	Dark olive brown precipitate.
Nitrate of silver -	A precipitate at first white, but quickly passing to yellow, red, and lastly to brown.
Sulphate of silver -	A dull white or stone coloured precipitate.
Oxinitrate of mercury	A white precipitate.
Oxinitrate of lead -	A white precipitate.
Supersulphate of copper	A dull white precipitate.
Muriate of bismuth	No precipitate.
Sulphate of iron -	No change.
Oxisulphate of iron -	The solution assumes a deep blood-red colour. No precipitate.

The liquid contained sulphite of potash and some other principle.

The effects of the sulphuric acid and of the muriate of barytes clearly proved the existence of sulphite of potash in the liquid; while that of the oxisulphate of iron indicated the presence of some other principle, to which the liquid was indebted for its peculiar characters. The separation of this principle in a pure state became therefore a necessary preliminary operation to its examination; after a few trials, I succeeded in effecting this separation. The following is the process I employed.

The liquid was evaporated by a gentle heat to dryness; upon the saline residuum alcohol was poured, till it ceased to extract any thing; by this means the whole of the sulphite and sulphate of potash was left behind, and the alcohol when filtered held in solution that part only, which had the red tingeing property with solutions of iron. The alcohol was now got rid of by distillation, and the salt it left in the retort was redissolved in water. This solution (B) gave the following results with the different metallic solutions.

This dissolved in alcohol, evaporated to dryness, and redissolved in water.

TABLE II. with Liquid B.

Action of metallic salts on this solution.

METALLIC SOLUTIONS.	EFFECTS.
Nitromuriate of platina	A precipitate similar to that in Table I, but in a smaller quantity, and longer in forming.
Muriate of gold -	Light olive precipitate, some gold reduced.
Nitrate of silver -	A grayish white precipitate, not changing colour.
Sulphate of silver -	A clear white precipitate.
Nitrate of mercury -	A copious white precipitate.
Oxinitrate of mercury	A white precipitate in small quantity.
Nitrate of lead	No precipitate.
Oxinitrate of lead -	No precipitate.
Superacetate of lead -	No precipitate.
Hyperoximuriate of lead	A slight white precipitate.
Supersulphate of copper	A dull white precipitate.
Muriate of tin	No precipitate.
Muriate of bismuth	No precipitate.
Sulphate of iron -	No change.
Oxisulphate of iron -	Same as Table I.
Oxisulphate of manganese	The crimson colour disappears; no precipitate.
Sulphate of Zinc -	No change.
Nitromuriate of cobalt	No precipitate.
Nitrate of nickel -	No change.

It is necessary to remark, that in the preceding Table, as well as in Table I, several of the nitrates and muriates were slightly reddened, though not in a degree to be compared with the oxisulphate of iron. I have not noticed this in the table, because I am not certain, whether this effect was not owing to a minute portion of oxide of iron, which might have been introduced into those solutions by the acids employed to make them, as both the nitric and muriatic acids of commerce generally contain some; an excess of nitric acid,

The solution a little reddened by some of the nitrates and muriates, acid, even if pure, might also cause this effect, as Table I may convince us. The solutions with which this effect occurred to me were those of bismuth, silver, mercury, lead, cobalt, gold, and platina.

not affected by the air.

The liquid B is not altered by exposure to the air.

State of the sulphate unimportant.

Its effect on oxisulphate of iron is the same, whether this sulphate is neutral, or contains an excess of acid, or is supersaturated with carbonate of ammonia.

Sulphuric acid destroys the colour, unless much water present.

Sulphuric acid destroys the colour produced on oxisulphate of iron, provided the three liquids are in a concentrated state. If there is much water present, no change ensues.

How is the principle formed?

Having obtained the tingeing principle B separate from the other salts with which it was contaminated, I asked myself, to what was its formation and the simultaneous disappearance of the prussic acid, during the second ebullition, owing? I could imagine but five causes for this that were likely to have been efficient, concerning each of which I made a question to be resolved by experiment, viz.

Question 1.

Was it owing to the complete separation of the oxide of iron from the triple prussiate by the sulphur, and the subsequent decomposition of the simple prussiate by the heat of ebullition long continued?

Question 2.

Was it owing to the action of the sulphurous acid produced?

Question 3.

Was it owing to the action of the sulphuretted hidrogen?

Question 4.

Was it owing to a combination of the prussiate of potash and sulphur?

Question 5.

Was it owing to the deoxidation of the prussic acid by the hidroguretted sulphuret?

Question 1 answered.

To answer the first question, it is only necessary to attend to the results afforded by long-continued boiling of the simple prussiate of potash. I shall state these results as I find them recorded by Professor Proust.

Results of long boiling the simple prussiate.

They are carbonate of ammonia, carbonate of potash, and some simple prussiate that escapes decomposition, even after four or five successive distillations. There is, therefore, no analogy between the products of this experiment and the liquid A; for had the latter contained carbonate of potash, it must have changed turmeric paper brown; had it contained ed carbonate of ammonia, it must have done the same, and likewise have given out ammoniacal gas when potash and lime were added; it must also have turned blue the solution of copper; and had it contained prussiate of potash, it must have produced prussiate of iron, when added to the green sulphate of this metal: but it will be seen by referring to Table I, that none of these effects were produced. Were farther evidence necessary of the dissimilarity of the two liquids, it might be mentioned, that Professor Proust poured alcohol on the saline residuum of his distillation of the prussiate, which took up a part that he found to be prussiate of potash; had any of the tingeing salt B been present, the alcohol must have dissolved this likewise, and it could not have escaped his observation. We have therefore ample grounds for negativing the first question.

In order to answer the second question, I passed sulphurous acid gas for a long time through a solution of triple prussiate of potash; the prussic acid was expelled, and sulphite of potash formed; but this sulphite was not mixed with any tingeing salt. On the supposition, that the disappearance of the prussic acid, in the liquid A, might have been owing to its having been expelled entirely by the sulphurous acid; and that the tingeing liquid resulted from the mutual action of the other principles, namely, the oxide of iron and hidroguretted sulphuret of potash; I subjected a mixture of these materials to long boiling, but could not by this means produce a liquid, that tinted oxisulphate of iron red. Sulphurous acid gas, passed through water in which Prussian blue was diffused, did not in the least affect this compound. These experiments completely refute the opinion, on which the second question was grounded. Question 2 answered.

To enable me to reply to the third question, I passed sulphuretted hidrogen gas for several hours through a solution of triple prussiate of potash, on which it was found to have no effect. Question 3 answered.

We shall be little disposed to allow, that there is any foundation for the fourth question, when we consider the circumstances of the last-mentioned experiment, in which sulphur in the state of the most minute division was offered to the triple prussiate, without any combination ensuing; Question 4 answered.

and

and also when we compare the effects of the metallic solutions in Table II with those which would ensue with liquids containing sulpur. But, if any doubt should still be entertained on this subject, the following experiment will perhaps remove it. Into a solution of prussiate of mercury throw some pieces of phosphuret of lime; the oxide of mercury of this prussiate will thus be reduced, and separated from the liquid, which is to be filtered. Some of this liquid poured into carbonate of iron turns it red, the red colour soon disappears, and a white precipitate begins to form; this white precipitate soon changes to green, and, if a little nitric or oximuriatic acid be now poured upon it, it becomes a perfect blue prussiate of iron. This experiment, in which a liquid turning a solution of iron red was produced without the employment of a particle of sulphur, goes very far to negative our fourth question; and when considered in conjunction with the preceding ones, we can hardly do otherwise than dissent from the supposition, which gave rise to that question.

Question 5 answered.

The prussic acid is deoxidated,

and may be recomposed by oxidation.

But if the experiment last adduced tends to refute the fourth question, it very strongly supports the fifth; for the changes of colour observable were undoubtedly owing to successive stages of oxidation by the contact of the atmosphere. In confirmation of this question, it may likewise be asserted, that the long boiling with the hidroguretted sulphuret is a powerful deoxidating process. But it will be said to me, if it is really true, that the prussic acid has been deoxidated by this process, you ought to be able to recompose that acid from the solution B by oxidation. This struck me very forcibly; and being anxious to give this last proof of the truth of my deductions, I attempted the recomposition of this acid by several oxidating processes for some time without success. I had at last, however, the particular satisfaction of succeeding completely by the agency of nascent hyperoximuriatic acid. The method I employed was the following.

Process.

A little hyperoximuriate of potash was put into the bottom of a glass tube. Over this some of the liquid B, mixed with a few drops of diluted sulphuric acid, was poured. The heat of a candle was then applied to the bottom of the tube, and as soon as a violent action commenced, the heat was withdrawn. By this process the prussic acid was reproduced, as was proved beyond the possibility of a doubt by the formation of blue prussiate of iron, when poured into a mixture of green and red sulphate of this metal. Blue prussiate may also be produced at once, by substituting for the diluted sulphuric acid a solution of green sulphate of iron with excess of acid.

Necessary precaution.

It is essential to the success of this experiment, in which the prussic acid is regenerated from the liquid B by the nascent hyperoxigenised muriatic acid, that the excess of acid remaining in the liquid, after the oxigenising process, should be neutralized by an alcali previous to pouring it into the solution of iron, which should likewise be perfectly neutral.

The new principle hitherto combined with potash.

Having thus succeeded in proving, that the tingeing principle of the liquid B was suboxidised prussic acid, my next object was to obtain this principle in a free state; for we must recollect, that we have hitherto considered it only in combination with potash, with which it formed a neutral salt. This circumstance gave me reason for supposing it an acid, and I therefore determined to attempt its separation by abstracting its base by a stronger acid. The following was the process I employed for the purpose.

Obtained as a separate fluid.

The liquid B was evaporated nearly to dryness, and put into a retort with diluted sulphuric acid; a receiver was then adapted to it, and about two thirds of the liquid distilled over by a gentle heat; what remained in the retort was sulphate of potash. The receiver contained a colourless liquid, with a faint, sour, disagreeable smell, and a decided acid taste. This liquor I have named, in conformity with the principles of the new nomenclature, *prussous acid*, and its salts *prussites*, of which the liquid B contained one in solution, namely the prussite of potash.

The prussous acid.

The effects of the prussous acid on the earthy and metallic solutions, as far as I have tried them, are noted in the following table.

TABLE III. with Prussous Acid.

Action of the prussous acid on earthy and metallic solutions.

CHEMICAL AGENTS.	EFFECTS.
Muriate of lime -	No change.
Muriate of barytes -	No change.
Muriate of gold -	The gold precipitated metallic.
Sulphate of silver -	Copious white precipitates.
Nitrate of silver -	
Prussiate of mercury -	No change.
Nitrate of mercury -	Copious grayish white precipitate.
Oxinitrate of mercury	Very slight precipitate, white.
Oxisulphate of iron -	Solution turns blood-red, no precipitate.
Nitromuriate of platina	No precipitate.
Nitrate of lead - -	No change.
Oxinitrate of lead -	Solution becomes red, but hardly any precipitate formed, unless heated, in which case a copious white precipitate ensues. The red colour disappears, a rapid action takes place between the two liquids, and some of the nitric acid of the solution is decomposed.
Hyperoximuriate of lead	A slight precipitate, probably of muriate of lead.
Supersulphate of copper	Solution becomes slightly turbid.
Muriate of bismuth	No precipitates.
Nitrate of nickel	
Muriate of tin	
Nitrate of cobalt	
Sulphate of iron	
Sulphate of manganese	
Sulphate of zinc	

Readier process for preparing prussiate of potash.

I cannot conclude this part of my memoir without giving a more simple and expeditious process for preparing prussite of potash, than that which I at first discovered. It is the following.

Pour a solution of prussiate of mercury into hidroguretted sulphuret of potash, till the mutual decomposition of the two liquids is completed; prussiate of potash is instantly formed, and may be separated by filtration from the solid combination of the sulphur and mercury.

Properties in the first process.

I wish also to observe, that the proportion of Prussian blue I have mentioned for boiling with the sulphuret is much larger than is necessary, as I have since succeeded in obtaining prussiate of potash when the proportion of Prussian blue was only equal to that of the sulphuret, but long boiled with the latter in two distinct and equal portions. The prussiate of potash thus obtained is, however, mixed with a much larger quantity of hidroguretted sulphuret than when a greater portion of Prussian blue is employed.

Use of this acid.

Whether the prussous acid can be applied to any use, time and future experiments must decide. It appears to me to be a very delicate test of silver and of iron in solution.

The prussic acid contains more oxigen,

The preceding experiments, by proving the presence of oxigen in prussic acid, give it a stronger claim than it before possessed for being placed among the acids.

but is less acid than the prussous.

The prussous acid possessing stronger acid properties than the prussic is a curious, though not a solitary, instance of the effect of oxigen in diminishing acidity, when its quantity exceeds a certain fixed proportion; in this respect the prussic acid is analogous to the oximuriatic.

Pure triple prussiate produced.

To recur to the attempt which gave rise to the researches that are the subject of this memoir, I beg leave to state, that I have succeeded in producing pure triple prussiate of potash, by stopping the process before the change which produced the prussite ensued; and by subsequent purification of the lixivium from sulphates and sulphites, by acetate of barytes; from sulphur by acetate of lead; and, lastly, from the acetate of potash thus formed by crystallization; but on account of the complication of this process, I hesitate to recommend it for general use.

ROBERT PORRETT, Jun.

Tower, London, April 21, 1809.

VI.

On the Oxides of Iron, and the Manner in which they comport themselves with some Acids. By Mr. Bucholz*.

Proportion of oxigen in iron oxides not settled.

THE little agreement among chemists with respect to the proportions of oxigen in oxides of iron induced Mr. Bucholz to make these experiments. He employed for the purpose bright iron wire of the greatest purity.

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* Annales de Chimie, vol. LXV, p. 202. Abridged from Gehlen's Journal by Mr. Vogel.

A. *Experiments to determine the Quantity of Oxigen in Red Oxide of Iron.*

Red oxide

1. On a hundred grains of the iron abovementioned, in the state of filings, an ounce of water was poured; and nitric acid at 1·265 was added gradually, till the iron was entirely dissolved.

contains 0·295 of oxigen.

The liquid being cautiously evaporated, and the matter having remained at a red heat for a quarter of an hour, its weight was increased forty two grains. This result differs from those of Lavoisier and Proust.

2. The same experiment repeated with this difference, that sometimes half an ounce of nitric acid, at others an ounce and half were employed, the results were the same.

B. *Experiments on the Black Oxide of Iron.*

Black oxide.

Mr. Bucholz made many attempts to obtain the black oxide. He tried calcination, the action of cold water on iron filings, and the precipitation of green muriate of iron by ammonia and by potash*. Though these alkalis, says Mr. Bucholz, did not produce me pure black oxide, their addition in small quantities to the muriate of iron taught me, that the first precipitate of a greenish white, and the second of a blueish green, consisted of black oxide of iron only retaining more or less muriatic acid; whence it follows, that these precipitates are not to be considered as distinct oxides.

Ammonia and potash precipitate muriate of iron, not free from acid.

Aqueous vapour converts red hot filings into black oxide

Water in the state of vapour being made to pass through iron filings heated red hot, a black powder was obtained, possessing all the characters of an oxide of iron at a minimum.

A hundred grains of this black oxide were boiled with six ounces of nitric acid at 1·265. The solution was complete. Being evaporated and strongly calcined in a crucible, a red powder remained, weighing 110 grains. The black oxide therefore consists of iron 0·77, oxigen 0·23.

containing 0·23 of oxigen.

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* Mr. Bucholz insists much on the smell of phosphorus, or of phosphuretted hidrogen gas, emitted when ferruginous salts are precipitated by ammonia.

Examination of the Red Oxide of Iron at a high Temperature.

A hundred grains of red oxide were put into a Hessian crucible previously weighed, and this was placed in a larger. The whole was kept at a white heat in a forge fire for three quarters of an hour. The bottom of the lower crucible was vitrified. The oxide was become a blackish steel gray powder, with a metallic lustre, attractable by the magnet. It had lost three grains.

Red oxide exposed to a white heat

The same quantity of red oxide was kept at a white heat for an hour and half. Its loss then was six grains and half. The lower stratum of oxide was completely fused, of a clean fracture, a steel gray, and a metallic lustre. The upper stratum was imperfectly fused, crystallized at the surface, and porous internally. It was a mixture of the red and black oxides of iron.

converted partly into black oxide.

Though a white heat causes the red oxide to pass to the state of black, there is no fear of a red heat producing this effect.

A red heat has not this effect.

Manner in which Iron and its Oxides comport themselves with Nitric Acid.

Nitric acid greatly diluted with water dissolves iron, and produces the nitrate in which iron is oxided at a minimum. Proust and Davy had already shown the same. This state however possesses little permanence, for if the liquid be heated ever so little, or the acid be not very weak, a nitrate at a maximum is obtained.

Action of nitric acid on iron,

The latter being boiled down quickly, set to cool, and cold water poured on it, a red substance insoluble in water, but soluble in nitric acid, was separated. This red substance gave out red fumes when calcined, and a red oxide of iron remained.

The nitrate of iron is not decomposed, when it is boiled down very gently. It leaves a thick residuum of a brown red colour, which attracts moisture from the air, and is perfectly soluble in water.

The red oxide of iron is soluble in nitric acid in a certain degree by means of heat. This action of the nitric acid however

and on the red oxide.

however is much more powerful when it acts on red oxide recently precipitated from a ferruginous salt by means of potash.

Sulphuric Acid and Red Oxide of Iron.

Action of sulphuric acid on red oxide of iron.

An ounce of green sulphate of iron dissolved in an ounce of water was made to boil, and six drachms of nitric acid, previously diluted with six drachms of water, were added gradually. A brownish yellow powder fell down, which, after desiccation, weighed 42 grs. [$\frac{7}{12}$ of a drachm, or 35grs. Eng.]. It was the neutral red sulphate of iron with excess of oxide mentioned by Mr. Thenard.

The supernatant liquor, of a reddish brown colour, was heated in a capsule; and immediately a brownish substance, similar to the preceding, separated, which however dissolved in proportion as the liquid evaporated. The thickened matter rapidly attracted moisture from the air, and resumed a sirupy consistence. Sulphuric acid immediately changed its red colour white. Evaporated to dryness with one drachm of sulphuric acid, and heated till all the excess of acid had disappeared, a white powder remained, which exhibited the following properties.

It was tasteless; and insoluble in water, either cold or boiling; but the latter deprived it of a little sulphuric acid, and changed its white colour to a brown.

On agitation with ammonia it changed to a brownish yellow, and the supernatant fluid contained sulphate of ammonia.

Muriatic acid dissolved it entirely by the assistance of heat, and exhibited a yellowish red liquid. Water acidulated with sulphuric acid had the same effect.

Here then we have a sulphate of iron of a white colour, in which the oxide is red and at a maximum of oxidation.

Recapitulation of the Facts in this Paper.

General results.

1. There are but two degrees of oxide of iron; the black oxide at a minimum, and the red at a maximum. No other state of oxidation exists in reality, and what has been taken for oxide has been nothing but a combination of one of these oxides with some acid, or a mixture of them in different proportions.

2. A

Oxides of iron.

2. A hundred parts of black oxide contain 77 of iron and 23 of oxigen. The red oxide is composed of 70·5 iron, and 29·5 oxigen.

3. The red oxide of iron, on being exposed to a white heat, is brought back to the state of black oxide: but a red heat does not change its nature, though it may alter its colour.

4. The change of black oxide to red is accelerated by calcination with caustic potash.

5. An oxide may contain one half of oxide at a maximum, notwithstanding it is strongly attracted by the magnet.

6. The black oxide is with difficulty soluble in nitric acid, requiring to be long boiled in concentrated acid.

7. The red oxide is not so insoluble in nitric acid as has been supposed.

8. Lastly, the red oxide may form different combinations with sulphuric acid; one of a reddish brown, and soluble; the other white, and insoluble in water, but soluble in acids.

VII.

Letter from Captain WILLIAM BOLTON *of the Royal Navy, on his improved Jury Masts, and on a Contrivance for better securing the Shrouds of Ships*[*].

SIR,

Advantages of captain Bolton's method of rigging jury masts.

FINDING some doubts were entertained relative to the practicability of rigging jury masts, upon the plan I had the honour to present to the Society in Oct. 1807, I beg you will lay before them a model completely rigged; from which it will appear, that the rigging so placed will give, from the angle being more obtuse, greater security, and supersede the necessity of cathur pinning [catharpings], and admit of the lower yards bracing sharper up.

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[*] Trans. of Soc. of Arts, vol. xxvii, p. 189. Some doubts having arisen as to the practicability of rigging Captain William Bolton's improved jury masts, described in vol. xxvi, page 167, of the Society's Transactions, (see Journal, vol. xxiv, p. 44) he in consequence has sent to the Society another model thereof completely rigged, which obviates all difficulty therein; and likewise has added another invention of his contrivance, for the better securing the shrouds of rigging; explanatory engravings of both are annexed, and the model is preserved in the Society's Repository. The gold medal of the society was voted to Captain Bolton for his naval improvements.

Improvement in reefing topmasts.

The hint which I gave formerly relating to striking a topmast is now carried into effect, by the mode in which I have fitted the topmast rigging, which admits of its being set up, almost instantaneously, at any point the mast shall descend to; the advantages of which are many, more particularly when the ships are riding hard in open roadsteads; for, by striking the topmast, ships will be enabled to ride much longer at their anchors, and still be ready to make sail on their topmasts, according to the state of the weather, which will or ought to determine the distance to which the masts are struck.

Improvement in setting up.

In this model I also present to the Society a plan, the earliest of my mechanical pursuits, for setting up a ship's lower rigging, which will be effected by one man; whereas in the ordinary way it is performed by an assemblage of tackles, and the labour of a whole ship's company: the larboard side of the model is fitted up according to the old plan; the starboard side * with my improvement, which consists simply of a screw attached to each shroud, and tightened by a nut under the channels, which should be well secured by iron clamps or knees for this purpose: the expense will be far less than in the common mode; and I do think the bare inspection of the model will be sufficient to establish its superior efficacy.

I will thank you to expunge the passage from page 168 in the last volume, after the full stop at *main-top* †, and substitute the following:

"The spare fore top-mast passes through a cap made from strong plank, *b*, into the square holes of which the heads of the two temporary masts above-mentioned are inserted, and the heel of the topmast is fidded on the tressle trees or top as in common, and the mast rigged as usually. The object of the strengthening cap, G, is to steady the spars, and also serves to fid the topmast on, if thought necessary."

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* The larboard side in fig. 1 of the plate.

† Journal, vol. xxiv, p. 45, line 13 from bottom.

I have the honour to be,

Your obedient humble servant,

WM. BOLTON.

Reference to the Drawing of Captain Bolton's Rigged Jury Mast, and his improved Mode of securing the Shrouds, Pl. IX, *Fig.* 1, 2, 3, *and* 4.

Explanation of the plate.

A A, fig. 1, represent the partners or pieces of timber, which are bolted to the quarterdeck for the mast to rest upon. B is the stump of the lower mast, which is cut square at the top, and of the same size as the head of the mast originally was; upon this square the main and spare lower caps *a a* are fixed; two mortices must be cut in the partners A A, to receive squares made at the lower ends of the two temporary masts D D, which are supported by the caps *a a*, one of them is a spare main topmast, the other a hand-mast; these two support the main top E, additional squares being made on the tressel trees to receive each of them. *b* is a cap shown in fig. 3, made of four-inch plank doubled for the purpose, and fitted upon the heads of the masts D D, for a fore topmast F F, the heel of which, when struck, rests in a mortice made in the stump of the lower mast; it is also steadied by a double cap G, separately shown in fig. 4, on which it fids; as it does finally on the top. The topgallantmast H is fixed to the topmast F by the top and cap in the usual manner. The figures 3 and 4 show the caps separated from the masts, and are the only things necessary to be made for the purpose; and the object of the cap, fig. 4, is to steady and prevent any wringing of the lower jury mast, and to fid the topmast whenever it is reefed. In the proposed method of securing the shrouds of ships in general, I represents the screws with loop holes for the shrouds to be lashed to, and K the screw nuts, which by means of a proper wrench may be easily turned, so as to tighten or slacken the shrouds at pleasure.

The other side of the ship is shown fitted up in the common way, that the contrast may be observed.

Fig. 2 is a side view of the mast and rigging, in which the same letters refer to the same parts.

VIII.

On a native Arseniate of Lead. By the Rev. WILLIAM GREGOR. *Communicated by* CHARLES HATCHETT, *Esq. F.R.S.*[*]

Native arseniate of lead not known,

§ 1. THAT the oxide of lead and the arsenic acid might be found in the state of natural combination, is a supposition highly probable, from the strong affinity which subsists between these two substances. But the existence of such a compound has not, as I conceive, hitherto been established by such proofs, as entitle it to be ranked among the *decided cases* of mineralogical science. I trust, therefore, that the observations, which I have the honour of submitting to the Society, on a new [†] ore of lead lately discovered in the county of Cornwall, so justly celebrated as well for the variety as for the richness of its mineral productions, will not be deemed superfluous.

till lately discovered

in a copper mine in Cornwall.

This mineral was raised in the mine called Huel-Unity, a very rich copper mine, in the parish of Gwennap. According to the information with which I have been favoured by Mr. William Davey, a very intelligent and experienced miner in that district, it was found in a lode south of Huel-Unity principal lode, at the depth of fifty fathoms below the surface, which lode underlay about two feet in the fathom south. At the depth abovementioned, this lode fell in, or formed a junction with another small lode or vein to the south, and when the junction took place, this lead ore was found. The veins of it are, in general, from six to ten inches wide, and they diverge on going west. Some particles of this lead ore have been found in the southern part, after the separation of the lodes; but the northern lode does not contain any, until the junction takes place. This ore is intermixed with some native copper, very rich gray copper, and black copper ore, and some is mixed with quartz. The walls of bóth veins are killas.

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[*] Philos. Transact. for 1809, p. 195.

[†] It is new at least to the miners in Cornwall; nor was there, previously to this discovery, any ore resembling it to be found in that splendid collection of minerals, which my valuable friend Philip Rashleigh, Esq., has so liberally formed, and as liberally employed in the promotion of science.

Description of this mineral,

§ 2. This mineral is regularly crystallized. The form of its most perfect crystals is a hexaedràl prism; they are of different sizes, from one tenth of an inch in diameter, to the size of a hair. The longest which I have seen do not exceed three tenths of an inch in length: these terminate in a plane, at right angles, with the axis of the prism; but the crystals of a smaller size are frequently drawn out into a very taper acumination, which appears to be a six-sided pyramid. A number of smaller crystals are often closely packed together in bundles, which are bent in different directions, and terminate in a point. The larger crystals either stand alone, or adhere, on their lateral planes, to the gangue, or are confusedly matted together in a mass.

Form of its crystals.

Some of them are hollow, as if an internal nucleus had been destroyed: and sometimes this internal nucleus overtops the external laminæ. The gangue is a white quartz, which frequently exhibits on its surface the appearance of a partial decomposition.

The red octaedral copper ore, and the copper into which that ore passes, are often intermingled with the crystals of this lead ore and inbedded in them.

Colour.

The colour of these crystals consists of a variety of tints of yellow. Some are of a beautiful wine yellow resembling the Brazilian topaz: this, in the greater number of specimens, passes into a delicate Isabella-colour: while, in other cases, we have the honey-yellow mingled with brown hues of different intensities: so that we meet with crystals resembling dark brown sugar-candy, or common resin.

Transparency.

Some of the crystals are beautifully transparent, while others possess this quality in part only, at their extremities, or in inferior degrees throughout their whole lengths.

Lustre. The external lustre, in some specimens, is vitreous; in others, resinous: but in some instances their surface is partially covered by tender and delicate filaments of a silky lustre. These filaments are sometimes found in a separate state loosely adhering to quartz; and they form a variety of this fossil.

Hardness. The crystals vary as to hardness. The angular fragments of the most transparent are sufficiently hard to scratch glass.

Tenacity. This mineral is easily reduced to powder, which has the appearance of pounded resin; it contracts a yellower tint by long exposure to the air.

Spec. gravity. The specific gravity of the purest crystals, taken at the temp. of 50° Fahrenheit, was 6·41.

Action of the blow-pipe on it, § 3. A fragment of crystal, exposed to the flame of the blowpipe in a gold spoon, melted into a brownish yellow mass, which on cooling did not assume any angular figure. It remained in a state of ignition apparently unaltered; but when a piece of it was exposed to the flame on charcoal, a rapid decomposition took place, arsenical vapours were extricated, and globules of a metal, possessing the common properties of lead, were left behind.

and of nitric acid. This mineral, in a state of fine powder, is soluble in nitric acid, even without the aid of heat. Care, however, must be taken, that it does not concrete into lumps. The vessel therefore which contains it must be frequently shaken, and the nitrate of lead produced must be, from time to time, dissolved in water, and poured off from the residuum. The process of solution is, however, accelerated by a digesting heat. Some silica remains, which, as the quantity of it is variable according to circumstances, appears not to be an essential ingredient of this fossil.

Nitric solution. The nitric solution is colourless; its transparency is not disturbed by nitrate of barytes. Nitrate of silver renders it turbid, and a small quantity of white curdly matter is deposited. Sulphuric acid and the liquid sulphates produce copious precipitates of a white heavy matter. If the fluid be poured off from this subsided matter, and it be freed from the superfluous sulphuric acid, by the means of nitrate of barytes, it will yield, on the affusion of liquid nitrate of lead, an abundant white precipitate, which, urged by the flame of the blowpipe on a support of charcoal, resolves itself into reduced lead and arsenical vapours.

These preliminary experiments led me to the probable Inference. conclusion, that this fossil chiefly consisted of oxide of lead, arsenic acid, and a small quantity of the muriatic acid.

§ 4. A. 1. Fifty grains, carefully selected from crystals Analysis. of a pale Isabella-colour, were reduced to a fine powder; and exposed to a low red heat for about an hour. Their weight was diminished by 0·15 of a grain.

2. The yellowish powder was now transferred to a vessel of pure silver, and mixed with a lixivium containing fifty grains of potash, prepared by the means of alcohol; a quantity, which I had previously ascertained to be sufficient to effect a complete decomposition of this mineral. The ley was gradually evaporated to dryness in a sand-bath. The soluble part was extracted by distilled water, and poured off from a yellowish white matter, which was sufficiently edulcorated (*a*).

3. Liquid nitrate of ammonia was now dropped into the alkaline fluid, as long as it produced any cloudiness: the clear fluid was now decanted from a small quantity of white matter, which had subsided, and rendered acid by nitric acid; ammonia, added to excess, produced a slight turbidness. These precipitates, after sufficient edulcoration, were added to the yellowish white residuum (*a*).

4. The liquid was now rendered slightly acid by nitric acid, and a solution of nitrate* of lead in distilled water was dropped into it, as long as it separated any precipitate. The clear fluid was poured off, and evaporated nearly to dryness, and a small quantity of white matter, thus obtained, was added to the former precipitate, which dried, and

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* If the colourless liquid *oxinitrate* of lead be dropped into a Oxinitrate of lead. dilute solution of arsenic acid, or of arseniate of potash acidulated by nitric acid, no immediate precipitation of an arseniate of lead is produced; but crystalline grains are, after a time, gradually deposited at the bottom of the vessel. But liquid *nitrate* of lead causes an immediate and abundant precipitate from these same dilute solutions. These two combinations therefore must be different.

exposed

Analysis. exposed to a low red heat, weighed, while still warm, 40·8, which, according to the proportion of 33 : 100, established by Mr. Chenevix, implies, 13·46 of arsenic acid.

5. The superfluous lead was now separated from the fluid by sulphate of soda, and filtered off. Ammonia precipitated a minute portion of flaky matter; it weighed, after ignition, 0·2 of a grain; it consisted of silica and oxide of lead, and must be attributed to the nitrate of lead employed.

B. 1. The yellowish white residuum (*a*) (A, 2.) was dissolved without effervescence in nitric acid, except a minute portion of silica, which, after ignition, = 0·28. A white heavy matter was thrown down from this solution, by liquid sulphate of soda. The clear decanted fluid was evaporated to a small volume, and sulphate of soda produced a farther separation of white matter. It was sulphate of lead, which, after exposure to a low red heat, and weighed, while warm, = 47·5; which, upon the supposition that one hundred parts of sulphate of lead contain 69·74 of lead + 3·48 of oxigen, are equivalent to 34·77 of oxide of lead.

2. The fluid, now freed from lead, deposited, on the affusion of ammonia, a greenish matter, which, after ignition, became red, and = 0·033 of a grain. It was oxide of iron.

C. 1. One hundred grains of larger crystals, some of which were hollow, and the surfaces of which were slightly and partially covered with silky filaments, treated in the same way, yielded 95·283 of sulphate of lead, equivalent to 69·76 of oxide; and 80 of arseniate of lead, which indicate 26·40 of arsenic acid. The oxide of iron, in this case, amounted to only 0·05 of a grain, and the residuary silica was in too small a quantity to be weighed.

2. I have endeavoured to decompose this fossil by boiling it to dryness in a solution of four times its weight of the purest subcarbonate of potash, and exposing the dry mass, for a very short time, to a low red heat; but I found, that only a part of the arsenic acid had united to the alkali; the larger portion of it was detected in the nitric solution of the residuum; but the relative proportions of the oxide and the acid were found to correspond almost exactly with the foregoing statement of them.

3 I found also, that carbonate of ammonia precipitated this mineral, *in an unaltered state*, from its solution in nitric acid; as no arsenic acid had united with the precipitant. The solution of the nitrate of ammonia was evaporated to dryness, and exposed to a red heat in a platina crucible; but nothing was left, except a slight trace of oxide of lead. We may infer from this the absence of both the fixed alkalis.

Analysis.

4. I found in one specimen only of this fossil any notable difference in the relative proportions of the oxide of lead and of the acid, to which it is united. It consisted of crystals confusedly matted together in a more compact mass, than this fossil generally assumes. One hundred grains were dissolved in nitric acid; the marine acid was separated by nitrate of silver, and any redundant silver by muriate of ammonia. The lead was separated by sulphuric acid, and the superfluous portion of that acid by nitrate of barytes, and the arsenic acid was combined with oxide of lead by the affusion of nitrate of lead. The muriate of silver = 9·8; the sulphate of lead = 97·6, and the arseniate of lead = 72, equivalent to 1·63 of muriatic acid, 71·46 of oxide of lead, and 23·88 of arsenic acid, respectively. The quartz = 0·35, and the oxide of iron 0·2 nearly.

Variety.

Another portion taken from the same specimen, treated with an alkali, gave very nearly a similar result.

D. It will now be necessary for me to speak concerning an ingredient of this fossil, which I may have seemed to overlook. I mean the muriatic acid: I have found some difficulty in ascertaining the proportion, which it bears to the other constituent parts, and from a cause, which I did not suspect. I considered, that the only sure mode of determining this point was to have recourse to nitrate of silver, which might effect a direct separation of the marine acid from the nitric solution of this fossil. But I found, in many experiments upon given quantities of this mineral, that the results, which I derived from this most valuable chemical test, were variable and uncertain.

Nitrate of silver as a test of muriatic acid varied in the quantity it indicates.

At last, I was enabled to trace the errour and uncertainty up to two sources. In the first place, I found that the muriate of silver was more abundant in the cases, where I employed

1st source of errour.

employed a vessel with a long neck for the solution, and *did not expose it to heat.*

I concluded therefore, that, when the process was conducted under different circumstances, the predominating *mass* of nitric acid produced its effect, and volatilized a portion of the muriatic.

2nd source of errour.

Another source of errour I found in the following anomalous circumstance, *viz.* a simultaneous precipitation of a portion of arseniate of lead takes place with that of muriate of silver. Whatever combination this may be, it is a weak one, and may be severed by nitric acid, which dissolves the arseniate and leaves the muriate; or by ammonia, which takes up the muriate, to the exclusion of the arseniate.

The conclusion, to which many experiments have led me, is this, that the muriate of silver, produced in the nitric solution of one hundred grains of arseniate of lead by nitrate of silver, amounts to about 9·5.

Proof that the acid in this mineral is the arsenic.

E. In order to prove, that the acid, which is combined with the oxide of lead in this mineral, *is the arsenic acid*, and that it is not combined with phosphoric, I decomposed some of its acid, which had been combined with lead in the foregoing experiments, by means of sulphuric acid, and filtered off the sulphate of lead. The fluid which passed through the filter was evaporated nearly to dryness, and it assumed the appearance of crystalline grains. Some of it was exposed to the flame of the blowpipe in a gold spoon; at first it became like a white dry powder, which melted before an increased heat: placed on *charcoal* and ignited, it was totally dissipated in arsenical fumes.

Some of it was dissolved in water, and dropped into liquid sulphate of titanium, a white precipitate was produced: combined with soda, it precipitated silver from the nitrate of silver, of a brick colour. It precipitated mercury from its nitrate, of a yellowish colour, which afterward became reddish. This precipitate, exposed to the flame of the blowpipe on charcoal, exhibited the same phenomena as arseniate of mercury.

Fallacious mode of distinguishing the phosphoric

I precipitated magnesia from its muriate, and redissolved it by carbonate of ammonia, perfectly saturated with carbonic acid. I divided this liquid into two portions, and dropped into *both* a solution of the combination of the acid of this mineral and soda. No precipitate was produced. I dropped into *one* of the vessels some liquid phosphate of soda, and a separation of saline matter was instantly produced. I soon, however, found, that this mode of distinguishing the phosphoric from the arsenic acid could not be depended upon. For in the other vessel, into which no phosphate of soda had been dropped, in a short time saline tufts made their appearance, and an abundant deposition of saline matter was formed. I found also, that if the solution had been more concentrated, the precipitation would have immediately taken place.

acid from the arsenic.

On making a comparative experiment with arsenic acid, I found, that it forms a triple salt with ammonia and magnesia, analogous to the phosphoric salt described by Dr. Wollaston. The figure of the arsenical salt, as far as I could determine it from a confused crystallization, is a triedral prism.

We are therefore, I think, authorized from the experiments herein detailed, to conclude, that the fossil, which is the subject of this paper, is arseniate of lead, and that, if we state that the relative proportion of the constituent parts of it are in one hundred as follows, we shall not be far from the truth:

Component parts of the mineral.

Oxide of lead - -	69·76
Arsenic acid - - -	26·40
Muriatic acid - -	1·58

The silica and the oxide of iron, which account for a portion of the loss; and the alumina and copper, which are sometimes found in an analysis of this fossil; I do not conceive to be essential to it.

The existence of a minute portion of muriatic acid as a constant ingredient of it is a curious fact: and it is still more curious, when we consider it in connexion with the analogy, that in this particular it maintains with the natural phosphates of lead.

Curious fact.

IX. *Experiments*

IX.

Experiments on the Combination of Phosphorus with Metals and their Oxides in the humid Way; to which is added the Examination of a Gas arising from a peculiar Decomposition of Alcohol: *by Mr.* THEODORE DE GROTTHUSS*.

Phenomena exhibited by solution of phosphorus in alcohol.

§ 1. It is well known, that the surface of water becomes luminous in the dark, when spirit of wine, in which phosphorus had been digested, is poured on it. Another curious appearance may be exhibited, by half filling a small phial with this alcohol, and putting it in a dark place, at a temperature of about 64° R. [176° F.] Even before the liquid begins to boil, a flame makes its appearance at the mouth of the phial, and reaches sometimes to the height of five or six inches, but is incapable of heating or setting fire to any thing. You may hold your finger in it some time without danger, and without feeling any heat. This flame exhibits to us an intermediate state between a simply luminous vapour, as is that of phosphuretted nitrogen, and an inflamed vapour, which in fact differs from it only by more rapidly decomposing the oxigen gas of the atmosphere.

Metallic phosphurets produced by it.

§ 2. Phosphuretted alcohol, poured into solutions of gold, silver, mercury, and copper, suddenly reduces these four metals, and precipitates them in the state of real phosphurets. Two drops of nitrate of silver, or of mercury, in a drinking glass full of distilled water, will be rendered very sensible by the addition of a few drops of phosphuretted alcohol, which immediately occasion a dark precipitate. The phosphuret of gold thus obtained had a considerable resemblance to the purple powder of Cassius, but differed from it in being of a deeper colour like that of indigo. When applied on porcelain, this appears neatly gilt on being exposed to the flame of the blowpipe, and a smell of phosphorus is emitted.

Phosphuret of gold.

Phosphurets of silver, mercury, and copper.

The most essential generic characters of the other phosphurets are: 1, to dissolve in nitric acid, with the evolution of nitrous gas, after which an insoluble phosphuret remains: 2, to emit a beautiful phosphorescent light in the dark, when thrown on a hot iron, and leave a metallic pellicle as a residuum.

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* Annales de Chimie, vol. LXIV, p. 19.

Their specific characters are easily determined, first by the metal left after the action of the fire, and secondly by that found in solution in the nitric acid.

Their formation explained.

Their formation may be explained in the following way. The spirit of wine impregnated with phosphorus quits it, as soon as it is mixed with the solution of either of the four metals mentioned above, and unites with the acid, producing ether. The metallic oxide, being set free, gives up its oxigen, either to the hidrogen of the alcohol, or to a small portion of the phosphorus, the greater part of which seizes on the metal in its pure state. This explanation results from the products of the two liquids, the phosphuret that falls down, and the ether that remains in the liquid, the fragrant smell of which is perceptible the moment after the operation is effected.

Singular agitation of the liquid.

§ 3. A very singular phenomena exhibited in this experiment is the agitation, which the metallic particles display. If, for instance, a few drops of phosphuretted alcohol be dropped into a solution of gold contained in a shallowish vessel, immediately we perceive currents take place throughout the whole of the liquid, and particles of the revived metal attracted in all directions. Sometimes they dart with impetuosity toward the sides of the vessel; sometimes a centrifugal force appears to whirl them round a common centre; and these commotions are always carried on with such velocity, that the eye can scarcely follow them. This exhibition is frequently rendered more pleasing by the fine prismatic colours displayed on the brilliant particles in motion. No doubt this decomposition of the light must be ascribed to a very thin stratum of some heterogeneous substance adhering to the metal, as in those coloured rings which Newton observed between convex lenses. I satisfied myself by a very simple experiment, that these movements were owing to the evaporation of the alcohol; for they ceased as often as I placed the vessel before a window, where the temperature was that day at 0 [32°F.]; and the moment I took it thence, and brought it near a gentle fire, the commotions began anew with great rapidity.

Prismatic colours.

The agitation owing to evaporation.

§ 4. The

Precipitates of tin, lead, bismuth, antimony, and platina, by phosphuretted alcohol.

§ 4. The precipitates produced by phosphuretted alcohol in solutions of tin, lead, bismuth, antimony, and platina, do not differ from those obtained by means of alkalis, and in some even by alcohol; except that the former are mixed with a little phosphorus, the separation of which hastens that of the oxide. The phosphorus is simply mixed with them however, for the colour of the precipitate is not altered by it; but we shall see presently, that this is greatly changed, as soon as a chemical combination of it with the oxide takes place.

Very different when the phosphuretted alcohol contains an alkali.

To effect this I boiled two parts of phosphorus with one of caustic potash in about six parts of rectified alcohol, the whole being contained in a small glass phial*. The greater part of the phosphorus was not attacked, and could be used in the subsequent experiments. After cooling, the liquid let fall an oxide of phosphorus of a very fine red colour, and the supernatant fluid became perfectly clear. It was this that was employed in the following experiments.

Nitrate of lead precipitated by this compound.

A portion of this phosphuret of potash in alcohol, being poured into a solution of crystals of nitrate of lead in water, precipitated a substance of a fine orange colour. Commotions took place throughout the whole of the liquid, and in a few moments black, shining, crystalline grains separated; which on examination comported themselves as metallic lead retaining a little phosphorus. The same solution of nitrate of lead, one portion of which I treated with spirit of wine impregnated with phosphorus simply, and another with caustic alkali, yielded in each case a white precipitate; while alcohol impregnated with phosphuret of potash immediately produced with it a very bright orange. I then attempted to give this fine colour to the white oxide thrown down by the alkali, by adding to it when separated phosphuretted spirit of wine, but in vain: which proves, that the orange-coloured substance is formed only at the moment when the phosphorus and oxide meet in a state of extreme tenuity favourable to their intimate union.

Properties of the precipitate.

§ 5. This orange precipitate easily loses its colour, sometimes

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* An examination of the gas evolved in this process will be found toward the end of this paper, at § 11.

times growing darker, at others becoming singularly white. To avoid the latter accident, which is owing to the volatilization and combustion of the phosphorus, it should be washed and dried cold. It then retains its brown red tint, and has the following properties. 1. Rubbed between the fingers it yields a smell of garlic. 2. Thrown on a hot iron, it emits the same smell, accompanied with a very beautiful phosphorescence, and leaving a residuum of phosphate of lead, fusible before the blowpipe, mixed with some white oxide. 3. *Acids expel from it no aeriform bubbles, but develope a nauseating and unequivocal smell of phosphuretted hidrogen.* 4. Nitric acid dissolves the greater part of it, but *without the least extrication of nitrous gas;* and the residuum is nothing but phosphorus.

Its nature.

From all these properties I conclude, that this substance is composed of oxide of lead, phosphorus, and a little hidrogen; or that it is a hidroguretted phosphuret of oxide of lead*.

Nitromuriate of antimony precipitated.

§ 6. Water, alkalis, and spirit of wine in which phosphorus has been digested precipitate the nitromuriate of antimony white; but alcohol impregnated with phosphuret of potash occasions in it a fine brown precipitate, which, being washed and dried without heat, remains unaltered, and has a considerable resemblance to kermes mineral. The mode in which this powder is formed; its phosphorescence by the action of caloric; its quiet and partial solution in muriatic acid, which produces with it the alliaceous smell; and the residuum of phosphorus; joined with the property of not decomposing nitric acid; evidently prove, that it is hidroguretted phosphuret of oxide of antimony.

The compound precipitates all metals, but does not always form phosphurets with their oxides.

§ 7. Though alcohol impregnated with phosphuret of potash precipitates all metallic solutions, it does not always form phosphurets of their oxides. In some cases the precipitates are oxides simply mingled with phosphorus, as occurs in the solutions of tin, zinc, cobalt, and manganese: in others they are phosphurets containing the metal in a pure state, and consequently decompose nitric acid; as those formed in solutions of silver, mercury, copper, and bismuth. I believe however, that beside the two phosphurets of oxides I have mentioned, others might be produced by repeating our trials, and varying the proportions of phosphorus and alkali, for these are of the greatest importance in preparing the two species I have mentioned. If, for instance, the precipitant contain too much phosphorus, or too much alkali, the precipitate will be white, both from the nitrate of lead, and from the nitromuriate of antimony; and we cannot obtain the orange precipitate in the first case, or the brown in the other, but by adding to the alcohol whichever of the two substances is deficient.

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* If this phosphuret have been some time exposed to the air, its oxide absorbs carbonic acid, and the nitric acid then extricates a gas, which the operator might at first be tempted to take for nitrous gas, but it has neither its smell nor its rutilant appearance.

The proportions of the precipitant important.

The action of sulphuret of potash exhibits a striking analogy with this. This sulphuret precipitates the nitrate of lead *red;* but, if it contain too much alkali, the precipitate is *white;* and if the sulphuret be sufficiently hidroguretted, it is *black*. In the latter case however the precipitate may be made red by adding an excess of acid to the metallic solution, for this extricates the hidroguretted sulphuret.

Sulphuret of potash analogous in its action.

§ 8. Mercury dissolved in nitric acid furnishes with the alcaline phosphuretted alcohol* a precipitate, that fulminates loudly when struck. As we do not always succeed in obtaining it equally fulminating in this way, it appears to me a more certain method, to prepare it by mixing phosphuret of mercury, obtained by means of phosphuretted alcohol (see § 2), with small dry crystals of nitrate of mercury.

Fulminating mercury.

§ 9. Though lead dissolved in acids is not separated in the metallic state by alcohol impregnated with phosphorus †, it is when its oxide is held in solution by alkalis. We have only to let fall a few drops of this precipitate into an alkaline plumbate, when the metal and the phosphorus will immediately unite, and separate in the form of small, black, brilliant, granular crystals. The presence of the alkali in this

Alkaline solution of lead precipitated metallic.

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[*] That must be employed, which gives an orange or red precipitate with solutions of lead; and it is essential, that the precipitate be washed only with cold water.

[†] This alcohol must not be confounded with that which contains, beside phosphorus, alkali likewise.

this case occasions the formation of a phosphite, and by this the attraction of the phosphorus for the oxigen is increased.

Classification of metals.

These observations suggest, that metals may be classed by their mode of comporting themselves with phosphuretted alcohol; and perhaps this system of division would be less equivocal than any other.

Sympathetic metallic inks.

§ 10. If we write on a piece of paper with solutions of gold, silver, mercury, and copper, and put the paper into a phial containing a bit of phosphorus, every letter will appear in a few minutes with the lustre of the original metal; and its brightness may be afterward increased by rubbing it with a polished substance, as glass, &c. It is sufficient even to leave the phosphorus a few minutes in the phial, after which it may be taken out; for the vapour emanating from it, though invisible, retains the revivifying power, and gives the writing the metallic lustre.

Mode of freeing water from oxigen gas.

Alcohol impregnated with phosphorus affords an excellent means of depriving water of the oxigen gas it commonly contains. For this purpose we have only to let fall a few drops into the water, and expose it to the light of the sun: the phosphorus will be oxided, without passing to the state of an acid, and the water may then be filtered, to render it clear.

Decomposition of Alcohol at no very high Temperature.

Decomposition of alcohol at a low temperature.

§ 11. When phosphorus and caustic fixed alkali are digested or boiled in rectified spirit of wine, this liquid is decomposed, and gives rise to the formation of water and of phosphocarburetted hidrogen gas, which may be collected in the hydropneumatic apparatus, and in which I have found the following properties.

Properties of the gas obtained.

a. This gas is invisible, and diffuses an alliaceous smell resembling that of phosphuretted hidrogen gas; with which however it cannot be confounded, as it does not take fire spontaneously, either in the open air, or when mixed with oxigen gas. I endeavoured to increase the proportion of phosphorus in it, by diminishing that of the alcohol and alkali; and then placed the gas in a heat of 70° R. [189·5° F.], yet it did not inflame on contact with the atmosphere.

b. When kindled by the flame of a candle, its burning is attended with vapours of phosphorus, part of which is precipitated in a concrete form on the sides of the vessel containing it.

c. It inflames with detonation, when mingled with oxigen gas.

d. Nitrous gas occasions no alteration in it: but if oxigen gas be added to a mixture of these two, a bright light appears, which is followed by a violent and very dangerous detonation. In the experiment I made on a fourth part of a cubic inch of the inflammable gas, enclosed in a glass tube which I had purposely selected for its thickness, the tube was broken to pieces, and the fragments flew up to the cieling of the room, where they made deep marks.

e. If three measures of oximuriatic acid gas be introduced in separate portions to one measure of this inflammable gas in a tube over water, it will take fire at the introduction of each portion, and burn with a very fine green light. The mixture expands at the commencement, and afterward diminishes prodigiously. All this goes on quietly, and there is no danger in the experiment, if we take care to use a pretty long tube.

f. It is injurious to the germination of plants, as appears from the following experiment. Having placed three parcels of the seeds of water cresses *(sisymbrium)*, moistened with water, under three jars, one of which contained oxigen gas, one atmospheric air, and the third this inflammable gas, I left them thus four and twenty hours. I then found, that the seeds in the first jar had begun to germinate, for they were soft and viscous; those in the second jar were less so; and those in the third not at all. The seeds were sown separately at the same time in good mould, and the celerity of their vegetation was in the same order. The seeds that had been in oxigen gas came up in three days; those that had been in atmospheric air, in four; and those that had been in the inflammable gas, not till the end of the sixth.

Analysis of it. *g*. To find the nature of this gas, I introduced fifty measures of it, and a hundred of oxigen gas, into Volta's eudiometer previously filled with limewater. After the inflammation of this mixture by the electric spark, the absorption was found to be at first eighty; no phosphorus was deposited; and on washing the residuum six measures more were absorbed. The sixty four remaining were nearly pure oxigen, for they were almost wholly absorbed by being fired repeatedly with proportional additions of hidrogen gas. The precipitate produced in the lime-water, being treated immediately with nitric acid, dissolved in it with effervescence: but it did not consist entirely of carbonate of lime; for caustic ammonia, poured into the nitric solution, separated a great part of it, namely the phosphate of lime. Hence we may conclude, that fifty measures of this gas require thirty six of oxigen gas to burn it completely; and that the products of this combustion are water, phosphoric acid, and carbonic acid: consequently it is phosphocarburetted hidrogen gas*. Its nature.

Residuum of the decomposed alcohol.

§ 12. The liquid remaining in the matrass, in which alcohol has been boiled with phosphorus and caustic fixed alkali, contains no phosphate, as we may satisfy ourselves by adding lime-water to it: but if the boiling have been long continued, phosphuretted hidrogen gas comes over toward the end, the bubbles of which take fire spontaneously on coming into contact with the air; and then the remaining liquid contains more water than alcohol, and a little phosphate. Hence it appears, that the presence of caustic alkali, more soluble in water than in spirit of wine, and the tendency of phosphorus to unite with hidrogen and carbon, give rise to the phosphocarburetted hidrogen gas, and the conversion of the alcohol into water, by subtracting the whole of its carbon, and the greater part of its hidrogen. It is remarkable, that the phosphorus, which remains after long boiling in the alkaline lixivium, remains in a fluid state at a temperature of 10° [54·5° F.] or less; but on throwing it into water, or into a vessel of any kind, it becomes

State of the phosphorus left.

A similar gas procured in a different way.

* Trommsdorff having strongly heated phosphoric acid with charcoal, obtained a gas, which he found to be a triple compound of hidrogen, phosphorus, and carbon. (See Van Mons's Chemical Journal, vol. II, p. 213 and 225.) There is every reason to presume, that his gas was the same as that I have examined, though the methods employed to obtain the two were totally different.

comes solid, and assumes the colour and transparency of the finest amber*.

§ 13. I cannot conclude this paper without mentioning some experiments I undertook, during my residence at Rome, on a species of glow-worm, the *lampyris italica* Lin., prodigious numbers of which are found in the summer in all parts of Italy.

Experiments on the light of the Italian glow-worm.

The light of this insect continues some time, when it is kept under water. It does not appear to be diminished for the first quarter of an hour, and is some hours before it ceases entirely. If oil be used instead of water, it is extinguished more speedily, the light ceasing in fifteen or twenty minutes. It is extinguished equally in hidrogen gas, carbonic acid gas, and in nitrous gas extricated in the hydropneumatic apparatus. If the insect be withdrawn from these gasses soon after its light is extinguished, its phosphorescence revives by the mere contact of the air; but when it will not revive even in oxigen gas, there is a mode of reproducing it with peculiar lustre. This consists in placing the insect, whether living or dead, in the rutilant fumes that arise from the mouth of a phial filled with nitrous acid. As soon as these vapours touch the abdomen of the insect, this becomes luminous, and diffuses a greenish phosphorescence, which speedily increases, till it becomes of a dazzling brightness; and afterward diminishes in proportion as it increased, till it entirely ceases. This beautiful phenomenon continues about a minute only; and though I have tried various means, I could never revive the phosphorescence after its extinction in nitrous vapour. If oxigen gas, or even atmospheric air, be passed up into a phial containing the insect and nitrous gas, the light will soon reappear with more vividness than in its natural state.

Fulminating lead.

P.S. After this paper was sent to the editors of the Annales de Chimie, I found a new property in the phosphuret of oxide of lead, which I had not observed before. Having slightly boiled five parts of phosphorus with one of pure soda in rectified alcohol, I poured this liquid still hot into a solution of crystals of nitrate of lead in water. The precipitate I removed from one piece of filtering paper to another, so as to deprive it quickly of its moisture, without allowing the oxigen of the air to act on it*. In this state it was of a dark colour, and possessed all the properties mentioned in § 5; but it had likewise that of detonating briskly, when the smallest quantity folded in paper was struck with a hammer. When the paper was opened after the experiment, I found it coated with lead completely revived. I also found, that touching it with a drop of sulphuric acid was sufficient to set it on fire.

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* I have noticed this phenomenon with soda in particular, and I had added gradually a larger proportion of this alkali than that I have mentioned of potash, § 4.

X.

Report made to the Physical and Mathematical Class of the French Institute on a Burning Mirror presented to the Class by Mr. Peyrard †.

Burning glass of Archimedes.

MR. PEYRARD, who has just published an elegant translation of the Works of Archimedes, was naturally led to reflect on the means, which that great geometrician is said to have employed, to burn the fleet of Marcellus before Syracuse. Both the ancients, and the authors of the middle age relate, that he used a burning mirror; but none of them enter into the particulars sufficiently, to gives us an accurate idea of his process. Anthemius, who built the church of Saint Sophia at Constantinople in the sixteenth century, and appears to have been a very intelligent architect, invented an assemblage of plane mirrors, to produce the same effect as that of Archimedes. Since that time Kircher, who perhaps was unacquainted with the works of Archimedes, thought of something similar. Lastly Count de Buffon constructed a burning mirror, composed of a hundred and sixty eight plane glasses; and the experiments, in which he employed it, are well known. These three processes, which come to the same thing, are attended with serious inconveniences.

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* The temperature that day was 15° R. (66° F.).

† Sonnini's Bibliothèque Physico-économique, Nov. 1807, p. 349.

Necessary construction.

For a mirror to reflect to one and the same point the rays of the sun, considered as parallel to each other, the reflecting surface must make part of that of a paraboloid of revolution, the axis of which is parallel to the rays of light, and its focus their point of union. If this mirror were composed of a great number of plane mirrors of moderate size, the plane of each must be parallel to a tangent of the paraboloid at the point where it is cut by the corresponding radius vector. Now in consequence of the motion of the sun the position of the axis of the paraboloid changes with some rapidity. If the form of the mirror therefore be unchangeable, the whole must turn round the focus with the sun, which appears to be impracticable: and if the parts that compose it be movable independent of each other, each of these parts must turn so as to be constantly perpendicular to the right line, that bisects the angle formed by the solar ray and the corresponding radius vector.

Impracticable with glasses fixed in a single frame.

With several glasses movable separately difficult.

It appears difficult to give the component mirrors the movement in question by means of a machine, less perhaps because the change in the sun's declination would render this machine complex, than because the expansion of the metallic rods, used for imparting the motion, would change in a perceptible and unforeseen manner the direction of the component mirrors; and because the action of the machinery would impart to each mirror a vibratory motion, that would keep the image in perpetual agitation.

Each glass managed by a different person.

There remains no other reasonable way therefore of composing a burning mirror of several plane mirrors, but by entrusting each of the latter to an individual, charged with keeping it in the proper position for reflecting the image of the sun to a determinate point, varying the position agreeably to the motion of the sun. But Mr. Peyrard justly observes, that this method is attended with an inconvenience, which must prevent its success. It is easy indeed for a single person, attentive and conveniently placed, to direct to a point the image of the sun reflected from a mirror of moderate size, and to keep it there, notwithstanding the motion

Inconvenience of this.

motion of the luminary. The difficulty would not be very great for three or four persons to do this at the same time. But if fifty, a hundred, or two hundred persons were employed to form a burning focus in this manner, as none of them could distinguish the image he sent from that sent by another, if one of the images alone should deviate from the focus, each of the cooperators would try whether it were his, and hence would arise an agitation and disorder, that would prevent the focus from being formed. This inconvenience Mr. Peyrard purposes to remove in a very ingenious way, by furnishing each of his mirrors with an apparatus not very complex, which we shall proceed to describe.

Contrivance for removing the difficulty.

A small telescope supported on a stand, and furnished with two wires crossing each other in the focus of the glasses, may easily be directed to the point, to which the image is to be conveyed. In this direction it is fixed by two screws. This telescope, without changing its direction, is movable on its axis between two collars, and can be kept in any position round this axis by another screw. On this telescope is fixed the mirror, which it carries with it when it turns round its axis; and which, independent of this motion, is capable of turning round another axis, perpendicular to that of the telescope. The telescope is to be turned on its axis, till the axis of the mirror is perpendicular to the plane formed by the incident and reflected rays, and in this position it is to be fixed by a screw. Lastly the mirror is to be turned on its axis, till the reflected rays are parallel to the axis of the telescope; and then the image of the sun must strike the object at which the telescope points.

First movement.

The two movements here mentioned are executed one after the other, and are capable of considerable precision. With respect to the first, when the axis of the mirror is perpendicular to the plane of the incident and reflected rays, the edge of the frame, which is perpendicular to the axis of the mirror, throws its shadow in a plane parallel to the incident and reflected rays, and consequently parallel to the axis of the telescope. This shadow therefore, or the boundary of the light reflected from the mirror, will cut an index projecting from the telescope in a right line at the same distance from the axis of the telescope as the edge of the frame is.

Accordingly this right line being traced on the face of the index, for executing the first motion it is sufficient to turn the telescope on its axis, till the shadow of the frame coincides with the right line on the index; which may be done with considerable precision.

Second movement.

For the second movement, it is clear, that, when the mirror is so placed as to have its reflected rays parallel to the axis of the telescope, if in the axis of the mirror, and close to the edges of the frame, a little line of the silvering be removed, the want of silvering will produce a shadow, that will fall on the middle of the right line of the index. This middle point being previously marked on the index, to execute the second movement it suffices to turn the mirror on its own axis, till the shadow of the unsilvered stroke falls on this point; which may be done with the same precision as the former movement.

Each person acts indepentantly of the rest.

Thus we see, that every person employed, however great the number, may direct the image he produces to the point assigned for the focus, without troubling himself about what is done by the others, and without being disturbed by their operations. It may be observed too, that the motion of the sun in its diurnal axis is not so rapid, but that one person might attend to ten mirrors near each other, and keep them in the right position, which would greatly diminish the trouble and expense of the process.

We are of opinion therefore, that Mr. Peyrard has carried the construction of burning mirrors composed of several plane mirrors to a degree of perfection, that it had not before acquired, and appears to us to merit the approbation of the class.

Done at the *Palace of the Arts*, *3rd of August*, 1807.

CHARLES,
ROCHON,
MONGE, *Reporters*.

The class approves this report, and adopts its conclusions.

DELAMBRE, *Perpetual Secretary*.

Paris, *4th* of *August*, 1807.

XI. *Analysis*

XI.

Analysis of the Bronzite. By Mr. KLAPROTH*.

A VERY remarkable fossil, found in large masses in the strata of serpentine near Kranbat in Upper Stiria, has been known within these few years by the name of bronzite. The following are its characters, as given by Mr. G. R. Karsten. Bronzite.

Its colour is a light tombac brown. Its characters.

It is in masses, disseminated in large pieces.

It has a metallic semibrilliancy.

Its fracture is lamellar, very distinct from simple splitting.

The pieces separated are large grained.

In thin plates, it is translucid: in the mass, opaque.

Where scraped it appears white.

It is semihard, and very brittle.

Its specific gravity is not great. That of the specimen analysed was 3·2.

A. Exposed to a red heat for half an hour its colour was rendered a little lighter, and it lost half a part per cent. Analysed.

B. *a.* 100 grains of bronzite were thickened with a lixivium containing 200 grains of potash, and then kept at a red heat for half an hour. The mass, which had not entered into fusion, was triturated in a mortar, then softened with hot water, supersaturated with muriatic acid, and completely dissolved. Being evaporated to dryness, and then treated with water acidulated with muriatic acid, the silex remained, which, after calcination, weighed 60 grains. Silex.

b. The muriatic solution was neutralized cold with carbonate of soda. The precipitate, treated with a boiling lixivium of potash imparted nothing to it. Well washed and heated red hot, 10·5 grs. of oxide of iron remained. Oxide of iron.

c. The colourless liquid, thus divested of iron, was made to boil, after which a sufficient quantity of carbonate of soda was added to decompose it entirely. The precipitate obtained, after being strongly heated, consisted of 27·5 grains of pure magnesia.

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* Annales de Chim. vol. LXV, p. 107. Translated from Gehlen's Journal.

Magnesia.

C. Sixty grains of bronzite were heated red hot with 300 grains of nitrate of barytes, till the nitrate was completely decomposed. The mass, being triturated, diluted with water, and supersaturated with sulphuric acid, was boiled for some time, and then filtered. The free sulphuric acid having been saturated in great part with ammonia, acetate of barytes was added. The supernatant liquid was poured off, and evaporated to dryness. The residuum was heated red hot, then elutriated with hot water, and the liquid filtered off. This liquid contained a trace of potash, for reddened litmus paper acquired a blue tinge from it after some time; but one drop of nitric acid was more than sufficient to destroy this alkalinity, and give the liquid an acid character.

Component parts.

Bronzite therefore is composed of

Silex	- -	60
Magnesia	-	27·5
Oxide of iron	-	10·5
Water	- -	0·5
		98·5

Occurs disseminated in serpentine.

The bronzite here described is the only instance known of its being found in compact masses; though it is frequently found in little separate pieces in serpentine; for instance, near Teinach, in the Pacher-Alp, in Lower Stiria, at Mount Hradicko in Moravia, at Zellerwalde near Siebenlehn, near Guanabacoa in Cuba, &c.

It is not yet decided whether the schillerstein (schillerspath, schillerblende) in the serpentine of Baste, near Hartzbourg, in the Hartz, may also be classed with it. As the analyses of this mineral yield alumine to the amount of 0·23 according to Heyer, and of 0·18 according to Gmelin, it should be classed with the schillernden hornblende, as has already been done by Mr. Karsten, if this large proportion of alumine be confirmed.

Hauy's diallage,

Mr. Haüy has made a separate species under the name of diallage, in which he has classed the smaragdite as green lamello-fibrous diallage, and the bronzite he gives as a variety of this by the name of bronzed metalloid lamello-fibrous diallage. On the contrary he separates from it the Labrador hornblende, of which he makes a distinct species under the name of metalloid, reddish brown, laminar hyperstène.

different from the bronzite.

In my opinion our bronzite cannot be ranked with the emerald, or the diallage, as it is of a different nature; for, according to the analysis of Mr. Vauquelin, the diallage not only contains a little magnesia and alumine, but a preponderant proportion of lime, to say nothing of the chrome. It is distinguished too by melting alone into a scoria before the blowpipe, while the bronzite is infusible.

XII.

Extract from a Letter of Mr. Gehlen, *on the Analysis of the Kannelstein and Greenland Garnet, on some Metallic Succinates, &c.*[*]

Analysis of some fossils.

In my way through Berlin Mr. Klaproth gave me the analyses of the kannelstein, Greenland garnet *(almandine)*, and haarkies (capillary pyrites, Brochant, II, 127). The first, you know, has been classed in the zirconian genus; and Mr. Lampadius, of Freyberg, imagined he found in it zircon and potash. Mr. Gruner, of Hanover, and Mr. Trommsdorf, of Erfurt, who have analysed the Greenland garnet, assert, that they found zircon in this also. Neither of these fossils however afforded Mr. Klaproth any of this earth, the following being the results of his analyses.

Kannelstein.

The kannelstein yielded -	silex - -	38·80
	lime - -	31·25
	alumine - -	21·20
	oxide of iron	6·50
		97·75

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[*] Annales de Chimie, vol. LXV, p. 185.

Greenland garnet.

The Greenland garnet	silex - -	43
	alumine - -	15·50
	magnesia -	8·50
	lime - -	1·75
	oxide of iron -	29·50
	oxide of manganese	0·50
		98·75

Capillary pyrites native nickel.

The capillary pyrites is not a sulphuret of iron, but native nickel, with a little cobalt and arsenic.

Succinic acid separates iron from manganese.

Benzoic acid supposed to do the same.

Oxalic acid preferable.

Several years ago, you know, in my examination of amber, and its acid, which I have not yet published, I found, that the succinate of iron was insoluble in water; that of manganese, on the contrary, very soluble; and that on this I founded an easy method of separating these two metals, which has since been much employed by Klaproth, Vauquelin, and others. The basis of this process being the different, or I may say, inverse solubility of two saline combinations, it was obvious, that other acids might produce a similar result. Accordingly Mr. Berzelius mentioned the benzoic acid, which however I cannot think very suitable, as Mr. Trommsdorff asserts, that the benzoate of iron is very soluble. Mr. John has lately employed with the same view the oxalic acid, and acidulous oxalate of potash; and Mr. Simon, an eminent chemist of Berlin, confirms their utility in a paper he has just written on the analysis of some fossils, the colophonite, scapolite, &c.; and he observes they are preferable to the succinic acid, because the oxalate of iron is less bulky than the succinate.

Titanium precipitated by tannin, not by gallic acid.

The same chemist has examined the phenomena exhibited by the gallic acid and tannin with titanium. This metal is not precipitated from its solutions except by tannin, or substances containing it.

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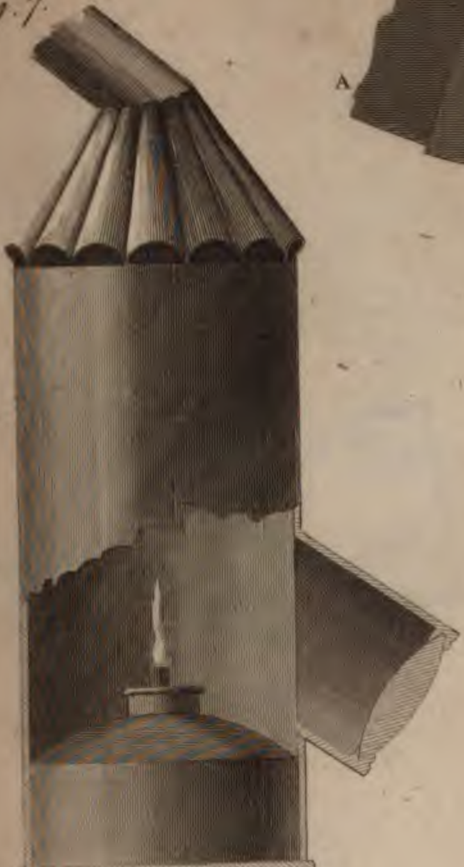

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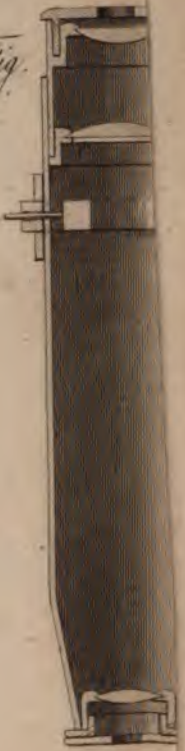

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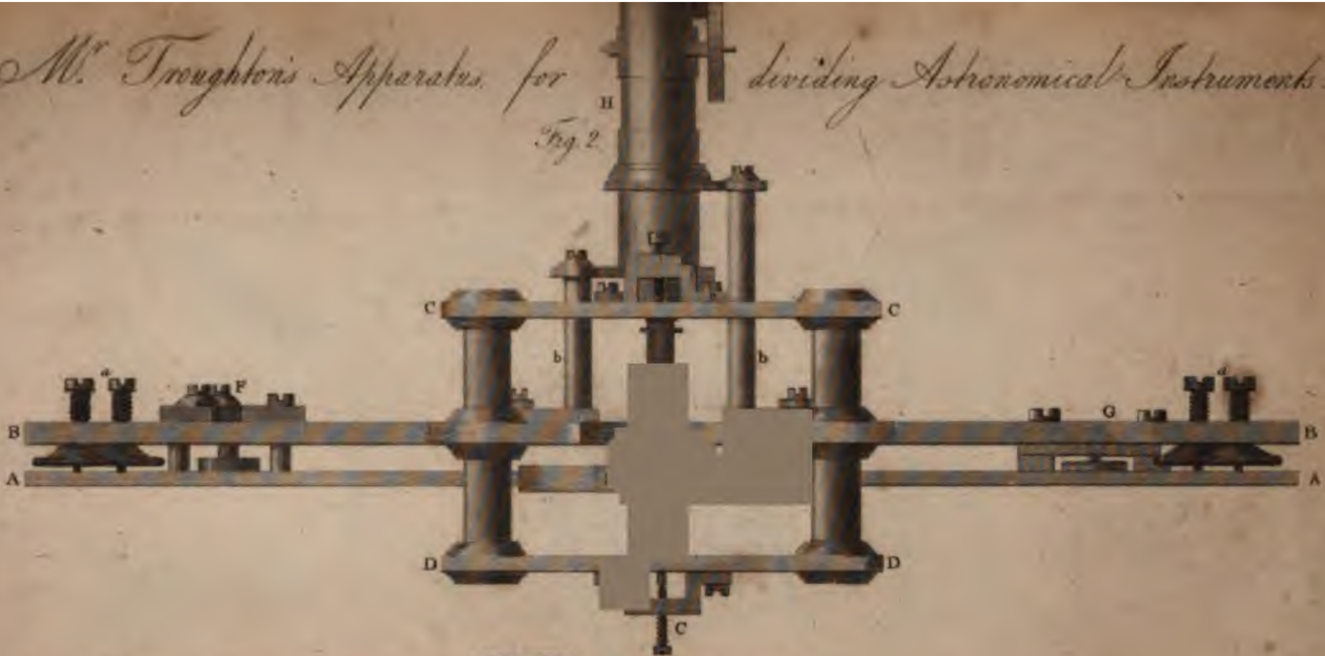

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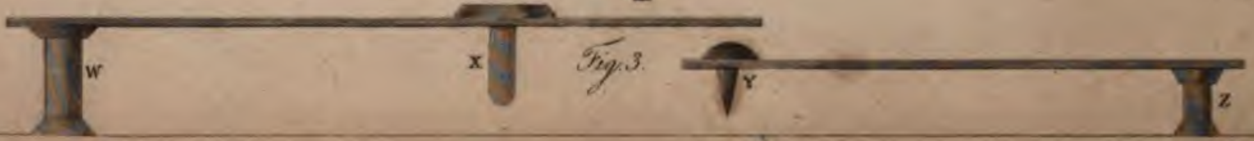

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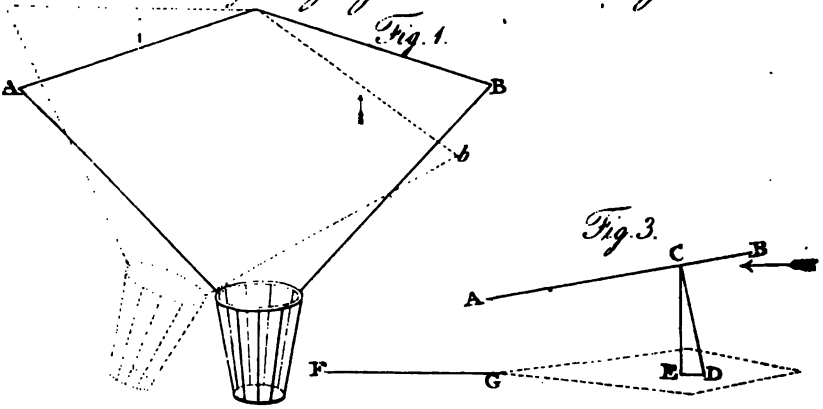

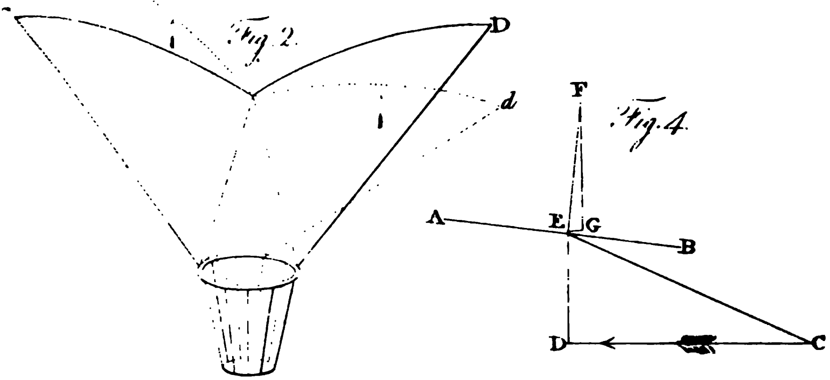

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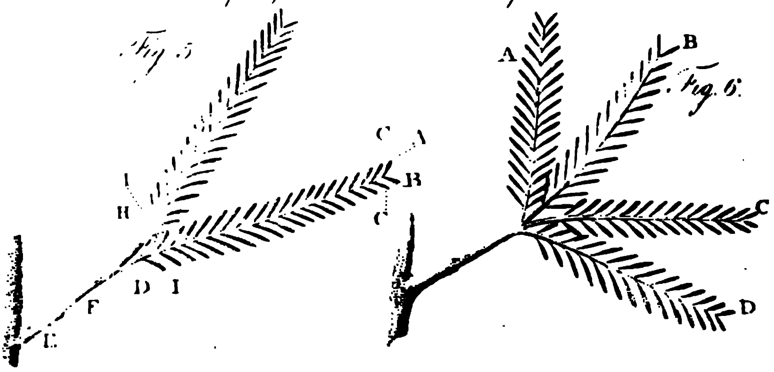

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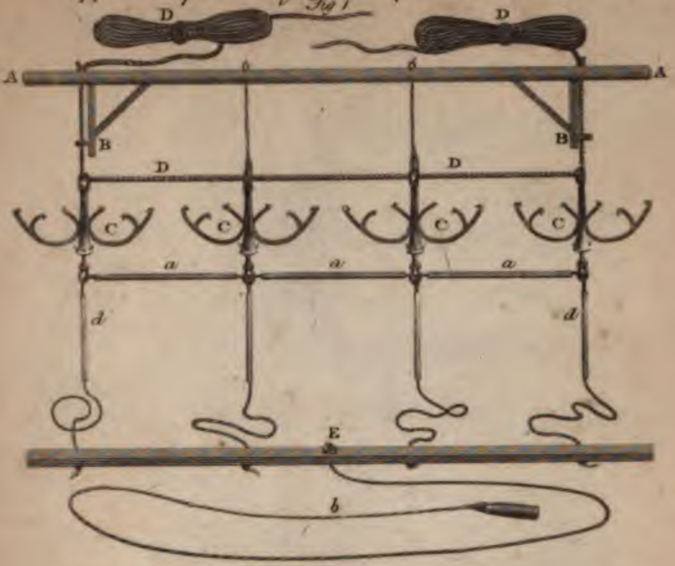

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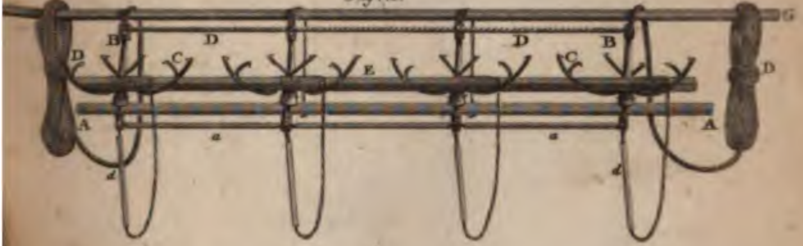

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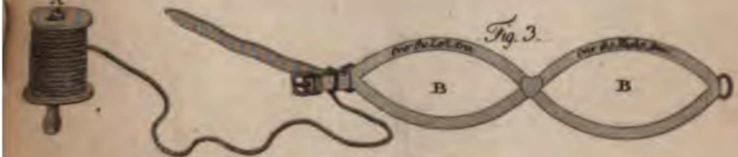

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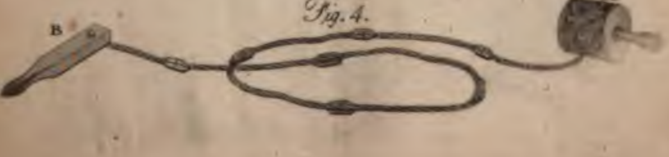

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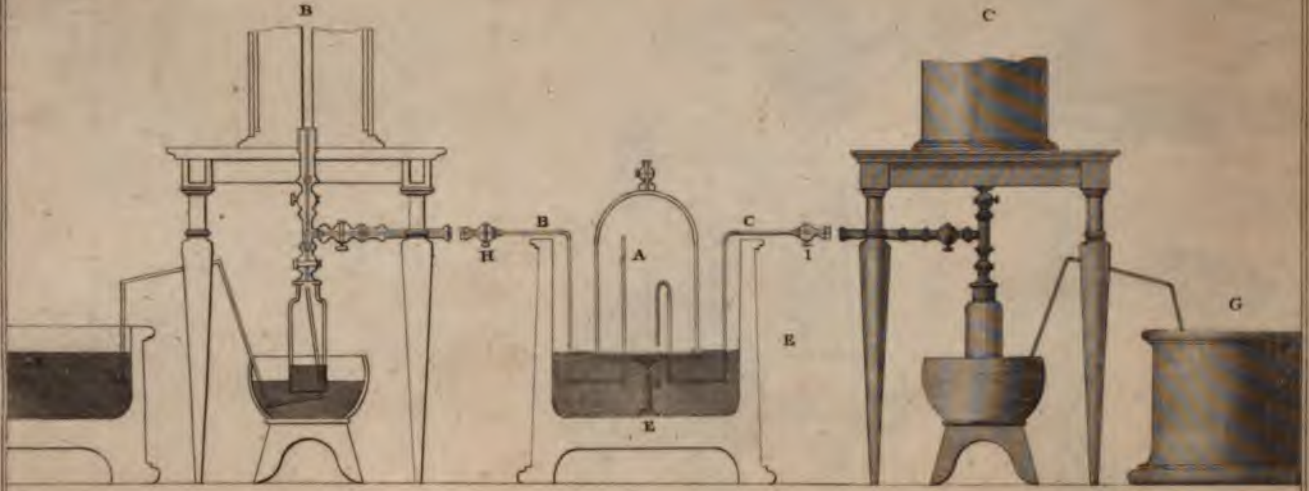

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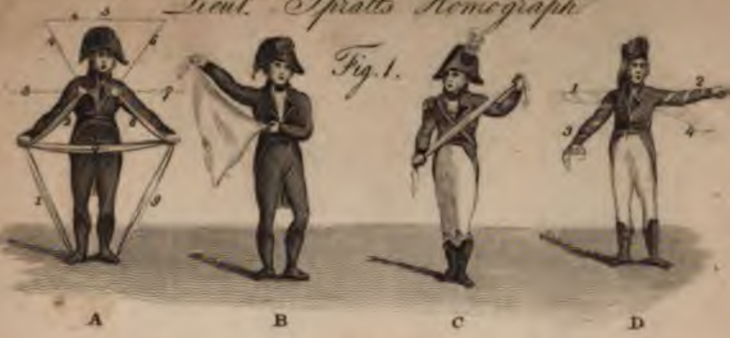

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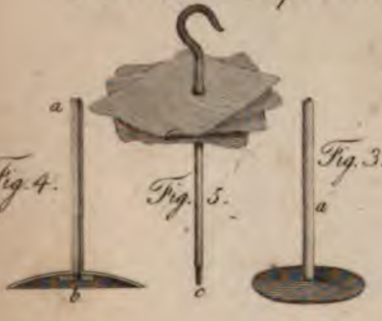

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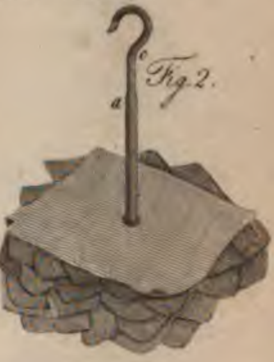

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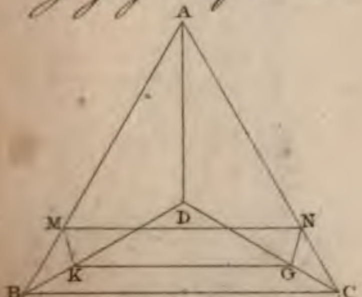

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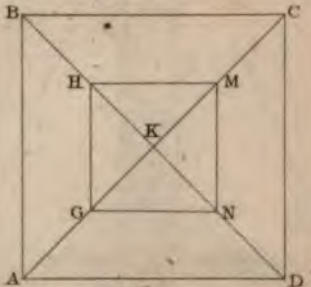

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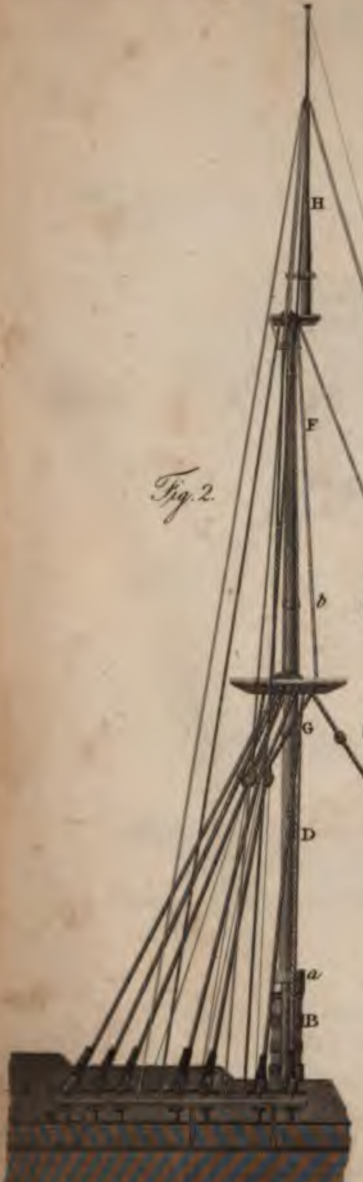

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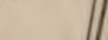

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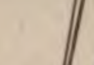

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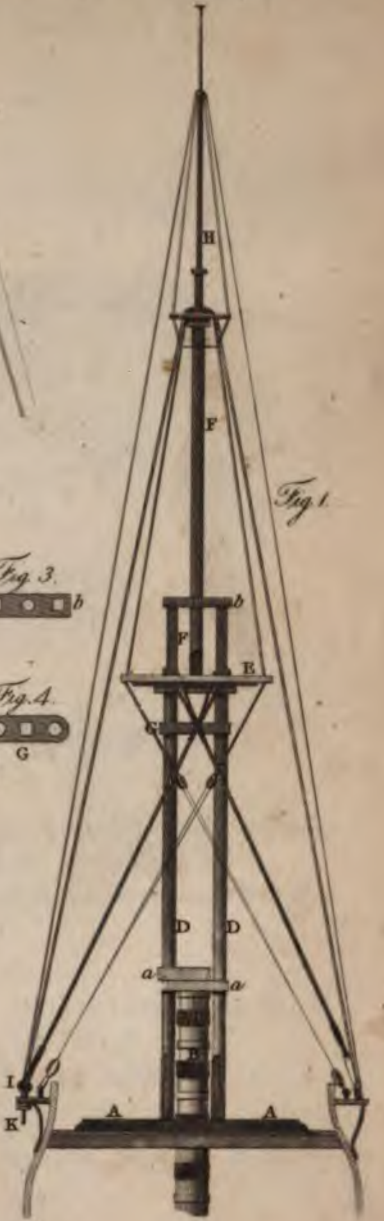

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OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS,

VOL. XXVI.

Illustrated with Engravings.

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LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR
W. NICHOLSON,
No. 13, BLOOMSBURY SQUARE;
AND SOLD BY
J. STRATFORD, No. 112, HOLBORN HILL.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1810.

ARTICLE I.

Remarks upon Meteorology. *By* J. Bostock, *M. D.*

(Concluded from Vol. XXV, p. 208.)

To Mr. NICHOLSON.

SIR,

Weather in September last.

I Now resume my remarks upon the weather in September last, in doing which I shall have occasion to bring forwards some of my peculiar ideas on the subject of meteorology. I shall, however, as much as possible avoid entering into any hypothetical discussions, my present object being merely to illustrate my method of making observations.

Form of cloud called an arc.

The beautiful evening of the 1st was succeeded, as I expected from its transparency, by a cloudy morning and falling barometer; rain came on about noon, and continued for two hours, and in the night there was a considerable squall of wind and rain. In the afternoon I have noticed a particular formation of the clouds, which I call *an arc*; it consists of a body of clouds, stretching in nearly parallel lines over a considerable part of the heavens, and converging in a point in the horizon; sometimes the lines converge at both ends, in opposite parts of the horizon, while at other times the lines shoot up to the zenith and terminate there. Upon the shape, extent, and position of these arcs, and upon their relation to the wind and the barometer, many meteorological predictions depend. In the first place as to their shape, they may be composed of long parallel lines or threads, forming *a linear arc;* or of small rounded clouds, lying side by side or in rows, *a mottled arc;* or they may be composed of clouds resembling a volume of smoke, as it rises from a chimney top, *a wreathed arc;* or they may assume the appearance of feathers, having a linear centre and lateral branches, *a feathered arc.* Then independent of any particular shape, the arc may be perfectly or imperfectly formed; it may reach only to the zenith, or it may reach quite across to the opposite point; it may be either increasing or diminishing in size, forming *a precipitating or dissolving arc;* or, according as it coincides with the present, future, or past state of the wind, it will take the title of *a present, future, or past arc.* *A present arc* almost always indicates, that the wind will leave the point from which it is blowing; and when it arises from a S or W quarter, we may form a pretty good judgment whether the wind will go to the right or left hand, by noticing whether the barometer be rising or falling; if rising, the wind will pass to the right hand, if falling, to the left; in employing the terms right and left, I suppose the face of the observer turned to the point of the horizon from which the arc proceeds. The appearance that was observed on this day was *a future arc,* and according to a pretty general rule, that whenever an arc is formed to the left hand of the wind, and the barometer at the same time falls, the wind will move into that point, we had in the evening a squall from E. On the 4th was another arc, which passed from S to N; an expression which signifies, that it had two converging points in opposite parts of the horizon, and that it was most distinct in the S end: had it been equally perfect at both extremities, it would have been expressed, an arc between N and S. As the wind and the arc crossed each other at right angles, the direction of the arc did not inform us, whether the wind

Indications of the wind from it.

would move to the right or the left; but there were several circumstances, which rendered it probable, that the wind would go towards S; the arc was strongly precipitating and there was a small *contiguous solar halo*, by which is meant, a halo that is close to the sun, and not one that forms a large circle at some distance from it. This combination of phenomena seemed to show, that there was a current of air passing above the wind in a southerly direction, and that this upper current was loaded with aqueous particles in a different state from those in the wind. When I first observed these appearances, I fully expected rain from the S; but when, after some time, I perceived, that, although the rapidity of the upper current increased, yet the lower one decreased and tended to a different direction, I concluded, that the commotion, which at that time affected the upper regions of the atmosphere, would not be felt in this district. Accordingly, only a few drops of rain fell, and for the next 36 hours there was nothing which could be called a shower. This is one of those cases in which comparative observations made in different countries would be extremely interesting; we might, by their means, trace the exact limits of a storm, and probably be enabled to ascertain the causes which immediately produced it.

Halo.

Comparative observations.

The 5th was what I call *a revolving day*, i. e. where the wind gradually moves round through the different points of the compass; a change which must be distinguished from that where it ceases in one quarter, and then springs up in a different one. The revolutions are either *direct* or *reverse*, i. e. in the same direction with the course of the sun, or contrary to it. A reverse revolution, such as took place on this day, is a very general sign of rain, whereas a direct revolution is what often takes place in the most settled state of the atmosphere; a rainy night succeeded. During this time there were many indications of the atmospherical electricity being in what is usually called a negative state, which generally takes place when the wind is E with a low and falling barometer, in opposition to the more usual kinds of E winds, which are accompanied by a high barometer, and a strong positive electricity. In cold weather this combination of circumstances is generally attended or succeeded by the

A revolving day.

Indications from it.

Atmospherical electricity negative.

Indications.

Sour day.

Shaded clouds.

Piled clouds.

the heaviest falls of rain, and in summer, by a close, oppressive state of the air, and thunder. The 7th was also *a negative day*, the barometer fell still lower, there were large thunder clouds at noon, and lightning in the evening. A different constitution of the atmosphere, however, now took place, it became dissolving, the barometer began to rise, and continued to rise with occasional interruptions for several days. This change of constitution, the commencement of a rise in the barometer, and the arc of the following morning, rendered it probable, that a complete change of wind was at hand, and it accordingly took place in the afternoon. The 9th was a different kind of day from any that we had had since the beginning of the month; I denominate it a sour day, a phrase employed by landscape painters, expressive of that state of the atmosphere, when the whole face of nature appears chilled and gloomy. The NW current was hastily filling up the partial vacuum that had taken place during the low state of the barometer, and being deficient in caloric, it produced that large body of clouds, which gave the peculiar character to the day. By the term *shaded clouds* I mean to designate the appearance which the heavens occasionally present, where the clouds are formed into rounded masses of greater or less extent, one side of which is very much darker than the other side, in the same manner that a solid globe is affected by the light shining upon it. By *piled clouds* is meant that state in which a part of the horizon is occupied by a quantity of large rounded clouds, which appear as if they were heaped one upon another.

Indications of unsettled weather.

The barometer had now attained nearly its average elevation, but the coldness of the air, and the peculiar appearance of the clouds, pretty certainly indicated, that the atmosphere was not in a settled state. If, after the influx of a cold current from the NW, the temperature of which is too low to keep its water in solution, the wind passes into a southerly point, rain almost always ensues, and the morning of the 10th exhibited a good specimen of the manner in which this operation is performed. In the early part it was bright, clear, and calm; but clouds soon began to form, which rapidly increased in bulk, and after some time were no longer able to support themselves. The lines which were observed passing from the W were, however, a proof, that there was still an upper current in this direction; and it was probable, from the little loss of weight which the atmosphere had experienced, that this current would finally prevail. The change took place in the afternoon, when the atmosphere exhibited an appearance of greater tranquillity than it had done for many days. For a few hours there appeared nothing to disturb the equilibrium, but the formation of a wreath of clouds, composing an arc from NW, indicated a current of air passing from that quarter, which, by its greater density, would probably rush into our region, and produce a new constitution. The state of the next thirty-six hours might be considered as the immediate effect of this NW current, although with us it assumed a westerly direction. There was a cold breeze, large clouds were flying along, which were dense and disposed to dissolve, evaporation went on rapidly [*], and I was much inclined to predict a favourable state of the weather, when on the evening of the 12th, the distant mountains were observed to be transparent, affording a pretty certain indication, that the wind would change to a precipitating point, and of course a strong presumption of rain. The following day was accordingly precipitating in a high degree; the atmosphere rapidly deposited its water, and the air rushed in to supply the partial vacuum that was thus produced.

Positive E wind.

On the 14th a constitution of the atmosphere took place, for which I was not prepared; a highly dissolving current of air proceeded from the E. As is the case with a *positive* E wind, the air soon became clear, and the weight of the atmosphere considerably augmented, but, according to a popular observation, which I believe to be very generally correct, a continuance of this state was not to be expected, both from the circumstance of its coming on so hastily, and also from its having been so immediately preceded by a S wind. The *tufts* that were observed, particularly those after the change, confirmed this opinion, for as they proba-

Tufts.

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[*] I have heard it remarked by old people, that, when evaporation goes on rapidly, the streets drying quickly after rain, more rain will soon follow; and I have frequently found it so. C.

bly depend upon partial currents of air, possessed of different properties from the prevailing one, so they lead us to suspect, that the mixture of these will produce some change in the equilibrium of the atmosphere. The appearances to which I give the name of *tufts*, are those clouds which resemble bunches of hair, the fibres of which are sometimes disposed in a perfectly irregular manner, and at other times lie nearly in a parallel direction. I believe, that, when these tufts point to any quarter, there is a current passing from that quarter, but there are many circumstances to be taken into account, before we can conclude that this partial current will become the prevailing one.

Indications of change.

The 15th, although calm and not unpleasant, had not that kind of calmness, which denotes a settled state of the atmosphere; but rather indicated the approach of some change in its constitution. The veering of the wind may be considered as an almost certain precursor of a change to a precipitating point, and a consequent diminution of the weight of the atmosphere. The appearance of the clouds rendered it probable, that there were at least two currents then prevailing, one from the NNW, and one from the E, while the appearance of the flocks led us to conclude, that the E current terminated in a stratum of air that was strongly precipitating. *Flocks* is a popular term, sufficiently expressive of a particular appearance of the clouds, when they form larger and more compact masses than those which I have called *tufts*; the fibres of which flocks are composed are also more generally parallel to each other than those of tufts.

Flocks.

The weather of the 16th, 17th, and 18th, may be considered as the result of that state of the atmosphere which was forming on the 15th; the barometer sunk considerably, the wind was in the S or W points, and sometimes high, with frequent rain, until on the evening of the 18th it proceeded to a violent storm. The atmosphere had lost three-quarters of an inch of its weight, and on the morning of the 18th it rained for several hours with a gentle breeze from SSE, at the same time that the clouds were moving rapidly from that quarter. Hence we might conjecture, either that there had been a partial storm to the SE, the effects of

Storm.

which

White clouds on a gray ground.

which would not reach us, or that to the N W there was a decomposition of some part of the atmosphere, commencing in the higher regions, to which the upper current, in which these clouds moved, was rushing. The appearances that afterwards took place proved, that this latter supposition was correct; for, although the wind went to the W, the diminution of the weight of the atmosphere still continued, while the appearance which I have described, of white clouds on a gray ground, is one of the most certain indications of the mixture of two currents possessed of different constitutions. It is to be observed, that, before the storm attained its greatest violence, the barometer began to rise; and as it depended merely upon the tendency of the air to produce an equilibrium of pressure, it was probable, that it would neither be of long continuance, nor very extensive.

Storms from diminution and increase of the atmosphere.

I am here led to notice the difference between this kind of storm, which is produced by an abstraction of part of the atmosphere, and that which appears to originate from a contrary cause, from a sudden increase of the volume of the atmosphere. Of this latter species a well marked instance occurs while I am writing this paper. Yesterday, March 25th, we had a strong gale from the E; the barometer had been before about the medium height, but it rose during the continuance of the storm; to day the wind is more moderate, and the atmosphere is becoming lighter. I may here observe how much insight would be gained with respect to the theory of the weather, had we a number of accurate comparative observations made in different places on the progress of such storms as the one that took place on the 18th of September. Were we to ascertain exactly at what hour the barometer got to its minimum, when it began to rise, how long after this storm acquired its greatest violence, when it began to abate, and when the barometer arrived at its maximum, we should probably have gone a great way towards obtaining a correct theory on the subject. One important point might be ascertained, whether a storm be retrograde or progressive in its formation. I apprehend, that, where the current is rushing forwards to supply a vacuum, the storm will be retrograde; i. e. supposing the partial vacuum to have taken place over the eastern part of the county

Comparative observations.

county of Lancaster, the storm, if proceeding from the W, will first be felt in this place, then it will recede to the coast of Wales, then to Anglesea, the Irish Channel, &c., until the equilibrium be established. But in the storm of yesterday probably the contrary course would be observed; the current being formed by an absolute increase of the volume of the atmosphere, it will push forwards like a current in any other kind of fluid, and will abate in proportion as the addition to the bulk of the atmosphere abates. If we pursue this idea so far as to suppose, that storms from a W point originate from the first, and those from an E point from the second of these causes, we may perceive why the former are more sudden in their commencement, are preceded by greater indications of commotion in the atmosphere, are attended with greater variations of the barometer, are more squally in their progress, more partial in their extent, and generally of less duration.

I shall not think it necessary to proceed any farther with the remarks upon my diary, as I conceive, that I have given enough to afford a complete illustration of my ideas on the subject. It is evident, that the first object is to obtain a full historical description of the successive changes of the atmosphere, to notice the periods of their commencement and duration, and the connection which they have one to the other. The second great point is to invent some method by which these changes may be accurately recorded; and it was here that I experienced the greatest difficulty, and that my diary will probably be regarded as most imperfect. The terms that I have employed will be thought very uncouth; and I have not unfrequently endeavoured to improve them, by forming a nomenclature upon scientific principles. I however finally determined to postpone the attempt, as I found it impossible to construct a nomenclature without involving in it more or less of hypothesis; and although it is difficult for any one to have thought so much as I have done upon the subject of meteorology, without forming an hypothesis, yet I do not consider mine as at present sufficiently mature to meet the public eye. It appeared therefore more advisable, not to attempt any thing like a methodical nomenclature, until there was a probability of its being founded upon well established

Objects in meteorology.

Terms.

blished principles. Not only is it desirable to avoid frequent changes, but from the influence which language exercises over our ideas, it is important to be on our guard, lest an incorrect set of terms should produce inaccuracy in our opinions. I am not unaware, that a scientific nomenclature for the appearance of the clouds has been attempted by Mr. Howard, but I hope I shall not be accused of presumption, if I give it as my opinion, that his set of names is much too confined to be of any great use, and that the hypothesis on which he proceeds is not entirely correct.

Mr. Howard's.

It will be perceived, that, according to my view of the subject, the science of meteorology must be advanced, not only by accurate observations of individuals, but by the comparison of observations made in different places. It was in a great measure from a conviction of this truth, that I have been induced to lay those remarks before the public, in hopes that some of your readers, in different parts of the kingdom, might cooperate with me in my plans. I should wish that your journal might be made the medium of communication, for there is certainly no other work, that on every account is so well adapted for this purpose.

Improvement of meteorology.

I am, Sir,
Your obedient servant,
J. BOSTOCK.

Liverpool,
Mar. 28, 1810.

II.

On some Improvements in the Electrical Machine. Communicated by Mr. J. CUTHBERTSON.

To Mr. NICHOLSON.

SIR,

SOME time ago I was informed by a friend of mine, that Professor Copland of Aberdeen had mentioned to him a valuable improvement, which he had made on my electrical machines. I requested my friend, to procure me the particulars

Improvement made in the electrical machine by Prof. Copland.

culars of this improvement; and he soon obtained for me the enclosed paper, which I believe, is in the Professor's own hand writing. I have made some practical remarks on this paper, which may be useful to electricians, and I have also added a hint of an extraordinary improvement I am about to make in the plate electrical machine. Should you approve these observations, you will oblige me by inserting them in your valuable Journal.

I am, with due respect,

Sir, yours &c.

54, Poland Street, Soho. J. CUTHBERTSON.

The negative power not equal to the positive

Mr. Cuthbertson's electrical machine is upon the whole the best I ever met with. The only fault I found was, that from insulating the whole machine, so many inlets were given to the fluid, that the negative power of it was by no means equal to the positive. His winch or handle is an insulating one, but it passed so near the mahogany pillar twice in every revolution, that a flash was seen from the handle to the pillar perfectly visible in the dark, and the effect of which I felt nearly to the shoulder. To remedy this I added a glass handle surrounded by a disc about seven inches diameter and of the same piece with the handle.

owing to the hand passing so near the pillar in turning the winch.

A glass handle and disc added to prevent this.

Within the hollow part of the handle was cemented a brass conical socket for receiving the steel pin of the former wooden handle, and a nut to prevent its coming off. The glass disc thus entirely prevented any supply of fluid to the machine by the hand. I now perceived however in the dark a constant stream of the fluid entering by the small brass ball and wire, which supports the silk flaps of the upper rubbers. I therefore took it away, and supplied its place by a solid glass rod.

Farther alteration.

The negative power of the machine was thus more than doubled, or it charged a vial negatively with less than one half of the number of turns it did when I received it. I indeed still perceived some appearances of light, at times, on the edges and angles of the wood work, but by rounding these more, or adding pretty large brass balls contiguous to them;

Edges and angles of the wood work rounded.

them; no light is now visible in the dark, and the negative power seems fully equal to the positive.

The rude sketch, Pl. I, fig. 5, will give some idea of the manner of fixing the handle with its disc. A brass cap is fitted, by grinding, over the inner nut and screw before cementing, to prevent their being fixed by the cement. It also serves as a reservoir.

P. COPLAND.

Observations on Mr. Copland's Paper.

The improvement had been employed before,

The machine described in the Professor's paper was not originally made for him, but for a gentleman who employed it principally for his amusement. I did not therefore attend particularly to its construction with regard to the perfection of either the positive or negative powers, and they might be imperfect. The Professor appears to think, that the greatest imperfection was the insulated winch which imperfection he has overcome by the application of a disc. This disc is precisely similar to an invention of mine made about 30 years ago, when I resided in Holland. I have given a description and plate of it in the second vol. of my *Eigenshappen der Electricitiet*, printed at Amsterdam, 1782; but on account of its awkward appearance, and risk of breaking, I left off using it, and have since employed a simple insulated winch, placed at a greater distance from the mahogany pillar of the machine, and this I find a more perfect remedy.

but disused.

Farther remarks on imperfect insulation.

With respect to the other improvements the Professor imagined he has made, and proved by experiment; I am inclined to think he has been deceived, because the negative part of an electrical machine, with such imperfect insulations as he describes, will charge a phial to a certain degree, with the same number of turns as one with the most perfect insulation. Such imperfect insulation does not begin to act till the charge attains a certain degree of intensity, till then it keeps pace with the charging power of a machine perfectly insulated. This circumstance does not seem to have occurred to the Professor; had he thought of measuring the advance of the charge at each revolution, before, and after, he had finished his alterations, the result would have been more satisfactory, and it might then have been known if he had made a real improvement, or not:

Intended improvement of the plate machine.

I shall now proceed to give an account of an improvement intended to be made in the plate electrical machine. I have often thought of attempting to improve the acting power of the plate machines, but in their present state they have given such general satisfaction, that I have deferred this attempt till an opportunity should occur of comparing their acting power with a cylinder properly constructed. All the cylinder machines I had met with were in some way or other defective in their construction, so that I was not satisfied with any of the comparative trials I had hitherto made; for in all these cases the plate machine had evinced very superior action. Some time since however Mr. Singer showed me a cylinder machine of his own construction. In this machine I could find no fault, and its acting power had been considered by most persons as very superior. I was at this time making a two-foot plate machine for Mr. Singer, and we agreed when this should be completed, to compare the acting power of the two machines. We met accordingly, Mr. Singer managed the cylinder machine, and I excited the plate. The result of a number of experiments proved, that the effect produced by any given number of turns was precisely the same with either machine, so that their acting powers were equal, but the force required to put them in motion was materially different; 8lbs. hung on the winch of the plate, when in a horizontal position, would move it; but it required 14lbs. to move the handle of the cylinder from the same situation. To complete a proper course of experiments, we found it necessary to construct some apparatus, and were therefore obliged to defer the continuation of these inquiries, till we had obtained the particulars alluded to. It may be necessary to state, that the diameter of Mr. Singer's cylinder is fourteen inches. Finding the power of the plate machine equalled by a cylinder, I was incited to attempt an improvement, and a contrivance soon occurred to me, by which I could double the acting power of the plate machine, or make a single plate act equal to a machine with two plates, or equal to two 14-inch cylinders, without being much more laborious

Cylindrical machines generally defective.

One of superior construction.

Comparative trial of this with a plate machine.

Their powers equal,

but the plate turned with less power.

The cylinder 14 inches diameter, the plate 24.

The power of the plate machine may probably be doubled, laborious to turn (I communicated this to Mr. Singer, when the same improvement occurred to him.) I am of opinion, that it is possible on this principle, to make one single 2-foot plate machine act equal to four cylinders, or to two double plate machines. If I succeed, I shall be able to equal the power of the large machine, which I made at Haarlem; and that too with one plate only and of much less diameter. If this contrivance should be applied to that machine, the effect must be astonishing; perhaps equal to the production of effects similar to the powers of the voltaic battery. In conjunction with Mr. Singer, I am now engaged in a series of experiments on this subject; should our success be equal to the expectations I have formed, the results will be of the highest interest to the progress of electrical science. When we have completed these experiments, we shall take an early opportunity of communicating them to the public, through the medium of your justly esteemed Journal.

or even quadrupled.

III.

Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition. By William Brande, *F. R. S. Communicated by the Society for the Improvement of Animal Chemistry**.

Sect. I. *Observations on Mucus, and on the Composition liquid Albumen.*

THE results obtained from the chemical analysis of the intervertebral fluid of the spalus maximus, an account of which is annexed to Mr. Home's paper "On the nature of the intervertebral substance in fish and quadrupeds†," led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state,

Experiments on mucus undertaken.

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* Philosophical Transactions for 1809, p. 373.

Philos. Trans. 1809; or Journal, vol. XXV, p. 214.

and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

Saliva agitated with pure water & filtered.

1. Saliva was the first source of mucus to which I directed my attention.

Solution contained saline matter.

In order to separate the albumen, which Dr. Bostock's analysis has shown it to contain*, it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

Saliva contained 0·02 of phosphate of lime and muriate of soda.

One thousand grains of saliva afforded, by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

Mucus of the trachea, and of the oyster.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol, and acids.

Tests of mucus act on the salts.

Finding, therefore, that the reagents employed to detect mucus† act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

Attempt to separate these

For this purpose, I procured three glass cups, each capable of holding rather more than a measured half ounce of

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* Nicholson's Journal, Vol. XIV, page 149.

† Nitrate of silver and acetate of lead. Vide Thomson's System of Chemistry, Vol. V, page 500, 3d edition; and Nicholson's Journal XI—251.

water

from the saliva by electricity.

water; one of these was filled with a mixture of equal parts of saliva and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a Voltaic battery of one hundred and twenty four-inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water*. By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state), and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

White substance at the negative side.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance surrounded, and adhered to, the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that, if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

White of an egg

To ascertain this point, an experiment was made on the albumen of an egg.

exposed to similar action.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which, reacting on the albumen, would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

The fluidity of albumen perhaps owing to an alkali.

This result I mentioned to Mr. Davy, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments, which were undertaken to establish so probable an opinion*.

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* It was conceived, that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

Coagulated albumen boiled.

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, showing that something is retained in solution.

Triturated in water,

Two hundred grains of the coagulated albumen of an egg were repeatedly washed and triturated in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue colour to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a dilute acid.

yielded alkali.

The solution evaporated.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the application of electricity, from sixty four-inch double plates, a copious coagulation took place at the negative pole.

Contained albumen.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

Alkaline solution of albumen separates from coagulated white of egg.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscid fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids†. This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

White of egg a compound of albumen, al-

It appears, therefore, that the white of an egg, in its fluid state, is a compound of albumen with alkali and water; that kali, and water.

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* On referring afterwards to Dr. Thomson's System of Chemistry (Vol. V, page 491), I find, that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

† Nicholson's Journal, Vol. XI—246.

when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chemical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution, which is thus formed, reacts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscid fluid already noticed.

Alcohol coagulates white of egg

The coagulation of albumen by alcohol and by acids may be explained by a reference to the principles already laid down.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquor was separated from the coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

by abstracting its alkali.

In this instance then, the albumen, in passing from the liquid to the solid state, gives it alkali to the alcohol*.

Acids do the same.

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

White of egg boiled, cut small, & washed with boiling water,

and the filtered fluid electrified.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another si-

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* When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

milar vessel containing a little distilled water, negatively electrified by one hundred four-inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

When the electrization had been carried on in this way for one hour the cups were removed, and their contents examined.

In the negative cup, soda;

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5·5 grains, which had the properties of soda, in a state approaching to purity.

in the positive, muriatic acid.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic crystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

Some muriate of soda.

This experiment shows, that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda*. We learn from the experiments of Mr. Hatchett, that minute quantities of other saline bodies are likewise present†.

In

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* May not a submuriate of soda exist in fluid albumen ?

Saline matters in albumen.

† After the destructive distillation of coagulated, dry, semitransparent albumen, there remained "a spongy coal of very difficult incineration ; "as towards the end of the process it appeared vitrified, and glazed with "a melted saline coat, which was, however, easily dissolved by water. "The residuum was again exposed to a long continued red heat, and "again treated with water, till, at length, a few scarcely visible particles "remained, which, as far as such small quantity would permit to be "ascertained, proved to be phosphate of lime. The portion dissolved "by water (which was by much the most considerable) consisted principally of carbonate, mixed with a small quantity of phosphate of "soda.

"Five

In the foregoing experiments, I had generally employed from sixty to three hundred four-inch double plates of copper and zinc, but in subsequent researches, made with a view of ascertaining the action of lower powers, the effects of which I shall afterward relate, I find that a battery of twenty-four three-inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

Small electrical power coagulates albumen.

SECT. 2. *Observations on the Composition of some animal Fluids containing Albumen.*

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

This test applied to other animal fluids.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali are produced at the negative pole. A large portion of albumen may therefore exist in a fluid, incapable of separation by heat, and in the present instance not to be detected even by acids, these reagents producing no effect on the filtered solution just alluded to.

Saliva. All its albumen cannot be coagulated by boiling in water, or by acids.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the Voltaic battery, a

Mucus of oysters contains albumen.

" Five hundred grains of dry albumen afforded 74·50 grains of coal,
" of which 11·25 were saline matter."

See " Chemical Experiments on Zoophites, with some Observations
" on the Component Parts of Membrane." Phil. Trans. 1800.

considerable and rapid coagulation takes place at the negatively electrified wire.

3. *Mucus of the Trachea, &c.*

Other mucus similar.

The other varieties of mucus, as from the trachea, the nose, &c., agree with the former, in affording abundance of albumen by electric decomposition; whereas scarcely any traces of this substance can be detected by the tests of acids, heat, or alcohol.

Alkalis and acids given out.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, showed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid in the cases of saliva, and mucus of the trachea and nose: the mucus of the oyster afforded only soda and muriatic acid.

Alkali apparently predominant.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids no traces of uncombined alkali (as in the white of egg) are to be detected.

Is mucus a compound of albumen with soda or its muriate?

These results lead to new ideas respecting the composition of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. *Bile.*

Bile.

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged throughout of a green colour, arising from the colouring matter at the same time separated.

Albumen in it variable in quantity,

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0·5 to three per cent, and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

and proportionate to the resin.

The

Soda, and muriatic and phosphoric acids.

The electro-chemical decomposition of this fluid affords, beside the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

5. *Milk.*

Milk.

In this fluid, the separation of albuminous matter at the negative pole is equally evident, though not so rapid, as in most other cases. The conductors from sixty four-inch double plates, highly charged, and immersed within four inches of each other in three ounces of cows milk, during one hour, produced the appearance of curds and whey, the principal part of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup were soda and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

Liquor amnii.

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

Its properties;

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphuretted hidrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalis produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

A dilute solution of liquid albumen.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen*.

7. *Pus.*

Pus.

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

The decomposition of albumen differs according to the power employed.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by Voltaic electricity takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty-four four-inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water), the coagulation was considerably more abundant at the negative than at the positive pole; but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four-inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

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* The difference in the results of the analysis given in the text, and that of VAUQUELIN and BUNIVA, most probably arises from the liquor of the amnios examined by those chemists not having been perfectly recent, and perhaps mixed with other secretions. See *Annales de Chimie*, XXXIII, p. 270.

IV.

IV.

Comparative Experiments on the Culture and Application of Kohl Rabi, Drum-headed Cabbage, and Swedish Turnips. Communicated by Mr. JOHN SADDINGTON, *of Finchley*[*].

SIR,

BEING actuated by the most patriotic motives, I beg the favour of you to lay the following communication before the Society of Arts, together with the plants herewith sent. I will endeavour to give you an account, with as much brevity as is in my power to render myself intelligible, of the nature of the soil, the mode of cropping, and the produce thereof. The plot of land being about two acres and a half, and lying on a dead flat, I obtained leave in 1805 to underdrain and break up the same, the grass being sour and useless.

Piece of bad grass land underdrained.

I cut two main drains, forty-two inches deep, gradually rising at top to twenty-eight inches, to give a sufficient fall, with sixteen branches twenty-four inches deep, rising to sixteen inches, terminating at top like the letter Y: the drains were wooded with elm, and laid with my own hands; this work was done in February. The soil is a loam, with clay and gravel under. On the 20th of March I sowed three bushels and a half of oats per acre, which produced thirty-nine bushels per acre, weighing forty-one pounds per bushel. The straw was used, as it was threshed, for litter to stalled oxen. The 28th of September seeded with winter tares, four bushels of seed per acre. Ate them off in May with sheep. Two fallow ploughings were given in June and August. About two hundred sheep were brought in at nights by way of fold. The 11th of October sowed three bushels of Thanet wheat per acre. Brined and limed in March, twice fed down with sheep. Produce, twenty-nine bushels per acre, weighing fifty-nine pounds per bushel, and very near three loads and a half of straw per acre. The stubble was mowed and cleared off, and the land got ready for

Soil.

Sown with oats,

and then winter tares.

Fallow ploughed twice.

Sheep folded.

Sown with wheat.

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[*] Trans. of the Society of Arts, vol. XXVII, p. 75.

Turnips sown.

for turnips. Three pounds of seed were sown the last week in August, when the plants were just making their appearance. Two quarters of gypsum were sown by hand to prevent the fly, which had the desired effect.

Gypsum to prevent the fly.

This proved a very valuable crop; having two hundred ewes which gave suck, it was a great acquisition to their milk. This induced me to try three experiments last spring with kohl rabi, or purple turnip cabbage, drum-headed cabbage, and Swedish turnips.

Comparative experiments.

Purple turnip cabbage.

With due respect I beg to recommend to the Society kohl rabi, as a prolific and nutritious plant for the feed of sheep and neat cattle; and green food may be produced by this means from October until May. To ensure a succession of keep, seed should be sown in March, April, and May. The plant bulbs above the ground; the leaf is much like that of beet; it will stand in defiance to the severest frost; and as a proof thereof, I have cut with my knife several of the plants through the crown two inches deep, and they have stood three months afterwards in a sound state; some of them are here produced. The plants may be transplanted like those of cabbage; many of those transplanted at eighteen inches apart, I have found to weigh ten and eleven pounds a piece.—I must now beg leave to introduce my method of cultivation, with the average weight of the crop.

Method of cultivation.

On the 14th of May, I sowed four ounces of seed broadcast, and transplanted about forty-six poles therefrom, on the 18th of June, at twelve inches apart each way. The weight of a square pole is seven hundred and thirty-two pounds, on an acre fifty-eight tuns six hundred eighty-eight pounds, taking each plant to average three pounds. The beauty and regularity of this crop in my idea overbalanced the trouble of transplanting.

Drum headed cabbage.

I likewise sowed upon a bed in the garden, the second week in March, eight ounces of drum-headed cabbage. The fly and slug were very destructive to the plants. I transplanted them the second week in June, upon ridges thirty-six inches apart, the land being dunged at the rate of twelve loads per acre. Some of the cabbages weighed thirty pounds. I think the average about twelve pounds each, or twenty-five tuns eighteen hundred sixty-four pounds per acre. The caterpillar was very destructive. I have picked off in a morning as many as would fill a quart pot. Although the kohl rabi was planted near to the cabbage, I never saw a caterpillar upon any of the plants.

No caterpillars on the kohl rabi.

In the middle of June, I sewed the remainder of the field with Swedish turnips, but lost two thirds of this crop by the fly. One of the best of the Swedish turnips is here produced, in order to show the great superiority of the kohl rabi, as there is not that waste in being eaten upon the ground, as it bulbs above, and the Swedish turnip in the ground. When the sheep have eaten the turnip level with the ground, and scooped out the inside, the remainder serves as a reservoir for the dirt and filth. The produce of this field has been sufficient for nine score of suckling ewes with rowen for five months. I sent to market, at Christmas last, house lambs fattened with milk only, which weighed eleven stone and one pound each, alive, at eleven weeks old. Should the Society consider these observations worthy of notice, I shall feel myself happy in hearing from you.

Swedish turnips.

I am, Sir, very respectfully,
Your obedient servant,
JOHN SADDINGTON.

V.

On the Properties of Furze, or Whins. Communicated by Major SPENCER COCHRANE, *of Muirfield House, near Haddington, North Britain**.

SIR,

THE Society having honoured me, by publishing in their 25th volume, my communication, stating the advantages arising from the culture of poppies, and that seven ounces of fine salad oil were furnished by expression from two

Poppy oil.

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* Trans. of the Society of Arts, vol. XXVII, p. 77.

of

pounds of the seed†; I now beg leave to add, that I am informed, considerable quantities of poppy seeds have been lately bought up, in different parts of the country, and the expressed oil from them sold at the price of Florence oil; and that emulsions made from poppy seeds answer in every respect the purposes of those made from almonds.

Furze useful food for cattle.

The following communication may perhaps be deemed worthy the notice of the Society. It relates to the use of whins or furze. Its utility as food for cattle has been long known, though probably not sufficiently appreciated; but as a medicine I never till within a few years heard of it. My information was first received from a gentleman, who has been an officer in the army, a friend and relation of mine; he is seventy-five years of age, and in good health, and what he says may be depended upon. In October 1806, he informed me, that his sight had been much strengthened by drinking an infusion of whin or furze blossoms, dried in the sun in summer. The infusion is made from a tea cup full of the blossoms, in a tea pot, in the manner of tea, and the dose half a tumbler at night. He never had a cough since he first used it, which was fifty years ago; it acts as a diuretic, and by perspiration, and when the dose is increased, promotes sleep. In October 1808, he informed me, that he still continued the use of the whin tea, that he had no cough, and that his skin was remarkably fine and soft, which he attributes to its use.

Infusion of the blossoms strengthens the sight,

and prevents cough.

I have also used the whin blossoms with good effect myself, and can safely recommend them.

Epidemic cold of horses cured by young shoots of furze.

My friend supposes the young shoots of furze may answer, if the blossoms cannot be got. He informs me, that when an epidemical cold came from Germany, and destroyed many horses in England, the east wind continued six weeks, and the infection came over to Ireland, where he had the care of a troop, in so poor a village that he could get neither bran nor malt for mashes, which were ordered for the horses, with sulphur, after bleeding. That he ordered the men to cut furze, and directed them to give it to the horses, after they had beaten it well on the pavement: that at first they had to mix it with oats, but that in two days the horses devoured it like clover. That by these means he recovered them all, though every other troop lost two or three; and that his was the only troop in good condition at the review.

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† See Journal, vol. XXI, p 157: also vol. XIX, p 282.

I remain with esteem, Sir,
Your sincere and humble servant,
SPENCER COCHRANE.

VI.

Account of several Varieties of British Marble, produced from the Babicomb Quarry, near Teignmouth in Devonshire. By Mr. J. P. Hubbard, *Picket Street, Temple Bar*[*].

SIR,

Marble quarry in Devonshire.

AGREEABLY to the wishes of the Society, expressed in their list of Premiums, stating that they were desirous to encourage the marble of the quarries of this country, I herewith send fifty various specimens, all arising from one quarry, named the Babicomb quarry, in my possession, situate in the parish of St. Mary Church, near Teignmouth, in the county of Devon, and adjoining the sea. I beg leave to observe, that, though an attempt to introduce this article has once before failed, yet I am confident, if I should be so fortunate as to have my exertions seconded in such a way as the nature of the concern requires, a considerable benefit would ultimately result to the country at large, as well as to myself. The numberless obstacles which I had to encounter, during a period of two years, arising from heavy expenses, and local prejudices, must have damped my exertions, if I had not resolved at the onset to give it a decided trial. Perhaps no period could have offered so eligible as this, for the advantage of the enterprise, owing to the present enormous prices of foreign marble. I am sorry to see, daily, many unaccountable prejudices arise against most articles of the produce of our own country; but I hope

Foreign marble very dear.

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[*] Trans. of Soc. of Arts, vol. XXVII, p. 85.

time will remove them. It would be presumption in me, to attempt to vie with the finer articles of continental production in this line, but it cannot be denied, that the application of the marbles now produced will be useful, economical, ornamental, and worthy of encouragement. The advantages which would arise to the country at large from a general introduction of this article are very evident, and if I can be favoured with the patronage and support of the Society of Arts &c. towards accomplishing such object, they would meet with the warmest acknowledgments of many individuals beside myself.

Great variety of the British prepared.

I have already prepared a great variety of articles, such as chimney pieces, slabs, &c., of very large dimensions, of these marbles, which are now ready for inspection; and which will show, that I have entered into this business on an extensive scale.

I subscribe myself with great respect,
Sir,
Your obedient servant,
JOHN P. HUBBARD.

Plan of the Society to exhibit British marbles.

The specimens of marble sent by Mr. Hubbard to the Society were each of them eight inches high, six inches broad, and one inch thick, and polished on one face; such are the dimensions pointed out by the advertisement of the Society, in order that a regular range of British marbles may be fixed round the Society's Great Room, to show to the public what our quarries can produce. Mr. Hubbard's marbles were, on being received, referred to the consideration of their Committee of Chemistry, and the following additional information obtained respecting the quarry and produce thereof, viz.—

Extent of the Devonshire quarry.

That the quarry which produced the different specimens is twelve acres in extent.

Varieties.

That marble similar to each specimen can be distinctly procured.

Large blocks,

That Mr. Hubbard had then in his possession columns of red marble, eight feet long, and two feet diameter, and believed that they might be got ten feet long, and five feet diameter, and that blocks of other kinds might be got of large sizes.

That he had at that time slabs six feet six inches long, by three feet six inches in width. and slabs.

That the quarry is close to the sea, and a part thereof covered by it at high water, and that he can load vessels direct from the quarry, having made a wharf for that purpose. Convenience of water carriage.

That the quarry is situate about four miles from Teignmouth, and was first opened about sixteen years ago, and was afterward neglected; but that it has been now worked by him for two years.

That the marble is harder in quality as the mine goes deeper, and that some part of it rises fifty feet from the sea.

That the sale price is about half that of foreign marble of similar appearance; that the general price is now about four shillings per superficial foot, and will probably be so reduced as to be delivered at three shillings in London. Price of the marble.

That it will take a finer polish than any other marble found in the kingdom.

That he supposes from sixty to one hundred workmen may be employed in the quarry next autumn.

That chimney pieces made from this marble are not injured from the heat of fire applied near to them, nor liable to crack from alternate sudden changes of heat and cold. Not injured by heat.

That great part of the refuse stones of the quarry will burn to lime, and that such lime is of superior quality to any other on that coast. Lime from the refuse.

The Society having taken into consideration the circumstance of Mr. Hubbard's having carried their views to so great an extent, and of his undertaking being likely to prove highly advantageous to this country, voted to him their Gold Medal, although no specific premium had been ever offered by them for coloured British Marbles. Gold medal voted.

Mr. Hubbard afterwards presented the Society with ten more specimens from his quarry, which with two specimens of Devonshire marble presented by Lord Clifford, and two others presented by Mr. W. Coles, have been framed along the 64 specimens.

the surbase of the Society's Great Room; where it is also intended to place such other marbles, the produce of the British Empire, as may be presented to them, with references to each sample, that the public may know whence each kind can be procured.

VII.

Inquiries concerning the Heat produced by Friction: by Dr. HALDAT, *Secretary to the Academy of Nancy*[*].

Heat an object of difficult research,

SO much has been done concerning heat in our days by eminent natural philosophers, that the subject would be exhausted, if it accommodated itself so easily to experimental research as many others; and if the fluid, which is pretty generally admitted as the cause of calorific phenomena, could be treated like those elastic fluids, the knowledge of which is at present so far advanced: but, incoercible in the highest degree, and incapable of having either its bulk measured or its weight ascertained, it eludes our research, and this real Proteus escapes into the depths of nature, the moment we attempt to lay hold of it. These properties however, which seem calculated to render it the despair of philosophers, have excited their emulation: but, as they exhibit themselves in different points of view, each has adopted for their explanation that hypothesis, which appears to him the most natural, and their opinions are divided.

and opinions on it divided.

According to the ancients an element.

The ancients explained the calorific effects, with which they were acquainted, by means of a fluid of extreme subtilty and incomparable activity, which gave it the power of attacking bodies, and resolving them into their first principles. They ranked this substance among the elements, of which they composed the universe. This opinion, variously modified at different times, was generally adopted till the age of Descartes; when that great genius, sent to renovate the sphere of science, represented these phenomena as a simple modification of matter, which all bodies were susceptible

This controverted by Des cartes.

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[*] Journal de Physique, vol. LXV, p. 213.

tible of, and which consists principally in the extreme attenuation of their constituent molecules. Philosophers then were divided into two classes, they who made the calorific phenomena to depend on the action of a substance of a peculiar nature, and which has been called matter of fire, or caloric; and those who supposed them to depend on a certain mode of being of the particles of bodies. The former opinion, which was most generally adopted, had become almost universal, since the phenomena of heat, having been more thoroughly studied, had been presented as dependant on a substance that might be transferred from one substance to another, in the same way as a fluid is poured from vessel to vessel; that might be combined with substances, and the combination of which produces remarkable changes; and lastly that might be set free from its combinations, and determined to others by the same means as are employed to produce similar changes in all substances. There seemed to remain no doubt of the substantiality of caloric, and philosophers seemed employed only in making better known a substance, the existence of which appeared to them sufficiently demonstrated, when count Rumford, dear to his country by his beautiful discoveries, and not less dear to humanity by his philanthropic labours, began to excite fresh doubts of the existence of caloric, and again place the phenomena ascribed to it among the modifications of which substances are susceptible.

The ancient opinion prevalent,

till opposed by count Rumford.

The property of friction to develope heat had long been known; but this fact, so deserving of attention, had not yet been subjected to proper examination. Count Rumford, having made a blunt borer turn in a brass cylinder immersed in water, obtained from it a quantity of heat so disproportionate to any thing the brass could have lost, that he thought himself warranted to infer, that this heat could not have arisen from any condensation of the metal, but must have been produced by the agitation of the particles communicated to the water in the manner of sound. This conclusion however, which tends entirely to overthrow the theory of caloric, has not appeared to be legitimately deduced from the facts; and Mr. Berthollet has refuted it in a note to

Heat produced by friction.

to his Chemical Statistics, vol. I, p. 247[*]. Thus the opinion of philosophers remains wavering between two theories diametrically opposite, and each supported by respectable authority. Accordingly I have undertaken the following experiments, with the view to add some facts to those, that will serve perhaps some day to elucidate more completely this important question. The experiments of count Rumford, executed on a large scale, were well calculated to give rise to conspicuous phenomena; but they appear to me, not to have been varied sufficiently. This is the object I proposed to myself, not merely the rubbing of bodies of different kinds, but likewise by varying all the circumstances that could concur in the developement of heat.

Acquires further experiments to detect its source.

Apparatus for this purpose.

The apparatus I employed consists of a small cubical oaken box, very firmly put together and cemented, in which an axis turns vertically. The lower end of this axis rests in a copper socket fixed to the bottom of the box, and on the opposite end a grooved wheel is securely fastened. In the upper third is a rim, resting on a bed of copper fixed to the cover of the box: and to the lower third is fixed a piece of copper furnished with rims, to retain cylindrical pieces of metal fitted to it. These pieces are 6 cent. 5 mill. [$2\frac{1}{2}$ inches] in diameter. On the convex surface of these hollow cylinders, the friction is produced by means of a spring fastened horizontally in the inside of the box. This spring receives at one extremity rubbers of metal, which are fitted to it by means of a groove; while a screw, passing through the box, gives the spring the tension necessary to press the rubber against the surface of the cylinder. A graduated arch fitted to the spring indicates in weights the force produced by its tension. The rotatory motion, that produces the continual friction, is given by an endless cord passing round the wheel on the axis and the large pulley of an iron-turner's wheel. The diameters of these are to each other as one to four, so that the velocity of the smaller is four times that of the larger, and by turning this only once round in a second, the smaller will make four revolutions in this period; so that the axis, moving with the same quickness, produces a friction, the velocity of which is more than 84 cent. [32·8 inches in the same space of time. This velocity, sufficient to produce sensible effects, is that which I have generally employed in my experiments. The other parts of the apparatus are hollow cylinders, equal in surface but of different materials, and rubbers of copper and of steel 3 cent. [1·2 inch high.]

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[*] So it is in Rees's New Cyclopædia, art. Caloric. *Trans.*

The heat produced raises the temperature of water in the apparatus.

The heat developed by the friction of the pieces of this apparatus is employed to raise the temperature of a mass of water of 3 dec. 664 cent. cubic measure [216 cub. inches], which the box is capable of containing, and this temperature is measured by the thermometer immersed in it. The water employed was in general nearly of the temperature of the air of the room, in order to avoid the influence this might have had on that of the water during the course of the experiment. This influence was farther diminished by preventing the air from being renewed, and shortening the period of the operations.

Exp. 1 with a brass cylinder and rubber.

Exp. 1. The first trial was made with a cylinder and rubber of brass. These pieces, before they were subjected to friction, were accurately weighed both in air and in water. Their temperature was 4° [39·2 F.]: the spring acted with a force equivalent to a pressure of 20 kil. [44lbs.]: and the mean velocity was 60 turns of the greater wheel in a minute. After 15 minutes continued friction, the temperature of the water was 6° [42·8]; and it was pretty regularly raised 2° [3·6°] in every interval of 15 minutes, so that at the expiration of 70 min. the temperature was 13° [55·4°]. The cylinder and rubber, weighed anew in air and water, showed no diminution in bulk or weight, that the balance could detect, though it was sensible to half a grain. The two pieces rubbed together however, exhibited a polish at the points in contact, which indicated a slight loss of substance. This was chiefly visible on the rubber. Thus a surface of 3 cent. [1·2 inch] broad, and 21 cent. [8·3 inches] long, rubbed with a velocity of 84 cent. [32·8 inches] per second, produced a quantity of heat capable of raising the temperature of 3 dec. 666 cent. cub. [216 cub. inches] of water 9° [16·2°]; or in other words of melting more than an eighth part of ice, though the quantity of metal detached from the surface was less than half a grain, and the condensation inappreciable.

Exp. 2. Cylinder of lead.

Exp. 2. For the brass cylinder of the preceding experiment I substituted one of lead. The two pieces were previously weighed both in air and water as before. The temperature of the air was 9° [48·2°], that of the water employed 7° [44·6]. The thermometer, observed every 15 minutes, exhibited the following progression in the temperature of the water 10°, 12°, 14°, 16° [50°, 53·6°, 57·2°, 60·8°]. The experiment continued 75′. Neither the leaden cylinder nor the rubber showed any perceptible diminution of weight or bulk. The point of contact was distinguishable only by a very slight mark on the rubber, and the polish of the surface of the cylinder: whence it appears, that, without any perceptible alteration in its bulk or weight, a cylinder of lead, the density of which is to that of copper as 11·352 to 8·788, produced an equal quantity of heat.

Produced an equal quantity of heat.

Exp. 3. Cylinder of tin.

Exp. 3. This result, the reverse of what the theory of friction seemed to indicate, induced me to make still farther inquiry into the influence of the density of the body rubbed by employing a metal of less density than that of copper; and accordingly I substituted for the preceding a cylinder of tin, the density of which is to that of lead as 7·291 to 11·352. The circumstances being the same, the temperature of the air and water 11° [51·8°], the acquired temperature gave the following progression: 13°, 15°, 17°, 18° [55·4°, 59°, 62·6°, 64,4°]. Hence it follows, that in the same time a cylinder of tin gives but $\frac{7}{9}$ of the heat produced by one of copper, while the latter gives an equal quantity to that produced by lead, though its specific gravity is only about $\frac{8}{11}$ of this metal. The volume and weight were found not to be sensibly changed.

Produced only 7-9ths as much heat.

Exp. 4 Cylinder of zinc.

Exp. 4. A metal, the density of which is still less than that of tin, but the hardness of which is much greater, and which possesses little malleability, zinc, was substituted for this metal. Rubbed during the same time, with the same velocity, and under the same pressure, the temperature of the air and that of the water employed being 10° [50°], it exhibited the following progression: 12°, 14°, 16°, 18°, 20° [53·6°, 57·2°, 60·8°, 64·4°, 68°]. The zinc cylinder then, less dense than those of copper and lead, afforded a much larger quantity of heat.

Produced more heat.

Exp. 6 and 7. Brass.

Exp. 6 and 7. I sought to confirm the influence of pressure on the evolution of heat by two experiments, in one of which I made it four times as great as in the other. For this purpose I employed the cylinder of brass, and the rubber of the same metal; and the velocity being equal, the pressure was at first 10 kil. [22 lbs.]. The temperature of the water rose only 1° [1·8°] in 30 minutes. Having afterward rendered the pressure equal to 40 kil. [88 lbs.], the temperature acquired by the water in the same space of time was six times greater, or 7° [12·6°].

Quadruple pressure produced septuple heat.

Exp. 7. Brass rubbingagainst a steel file

Exp. 7. The cause, to which we ascribe the most general influence on the developement of the heat produced by friction, is the erosion of the surface rubbed, the particles of which are separated with violence. In order to ascertain this influence, I employed a rubber of steel, cut so as to resemble a bastard file; and applied it to the surface of the cylinder with a force equal to 20 kil. [44 lbs.]. Having accurately weighed the cylinder in the air, I turned it with the same velocity as in the preceding experiments; and in the space of 60 minutes the temperature of the water was raised only from 14° [57·2°] to 18° [64·4°]. The cylinder had lost 3 decagr. [463 grs.] of its weight. This quantity of metal, a thousand times as much as that lost by the friction of the same cylinder against a rubber of smooth polished brass, in Exp. 1, gave but half the quantity of heat in the same time, though taken off by an equal pressure.

produced but half as much heat as when rubbingagainst smooth brass.

Exp. 8 Brass, surrounded by a bad conductor of heat,

Exp. 8. In the 8th experiment I proposed to ascertain the influence, that the free communication of the parts of the apparatus with the surrounding bodies by means of good conductors of heat, or their insulation by bad conductors, might have on the production of heat. With this view I placed the apparatus in a deal box, in which it was kept at the distance of a decim. [3·9 inches] from every side by pieces of wood half charred. The interval between the two boxes was filled with smallcoal, forming a stratum on all sides near four inches thick. I employed the brass cylinder and rubber, subjected to the same pressure, and rubbed with the same velocity. The time of the experiment was divided into three equal portions of 30 min. each. In each of these periods the temperature acquired by the water in heated more slowly.

the inner box was pretty regularly 3° [5·4°]. Though the communication with surrounding bodies was alternately interrupted and restored, it is to be observed, on comparing this experiment with the first, that, if the quantity of heat were equal, it was in a third longer time; whence it follows, that in the same time it was a third less.

Experiment 9. Brass insulated.

Electricity appears to have no effect in the production of heat by friction.

Exp. 9. The electric fluid, as well as caloric, is developed by friction, and generally progagated with more facility; and as these two fluids have some analogy to each other, the former may be suspected, to have some influence on the developement of the heat, and to furnish the matter. To appreciate this hypothesis, I repeated the preceding experiment, insulating the apparatus by nonconductors, and alternately establishing its communication with the ground. For this purpose it was placed in a box of very dry deal coated with resin, from every side of which it was kept at the distance of a decimetre [3·9 inches] by pieces of wood baked in an oven, and immersed while hot into boiling gum lac. The whole was placed on an insulating stool with glass feet. The experiment, which continued an hour, was divided into four equal portions of time, in which, though the communication with the earth was alternately interrupted and restored, the quantities of heat appeared equal. In the 60 min. the water acquired 6° [10·8°], whence it would appear, that insulation, either by nonconductors of electricity, or by bad conductors of heat, diminishes the quantity of the heat produced by friction.

Exp. 10. An iron wire compressed by repeated blows.

Exp. 10. Of all the causes suspected to influence the production of heat in our experiments, none appears more powerful, than the condensation of the particles of bodies arising from the pressure necessary for the friction. It is to this cause too, that Mr. Berthollet has thought fit to ascribe it; but as its influence rests solely on theory, I attempted to confirm it by experiment. For this purpose I constructed a small oak box, capable of containing a cubic decim. [61 cub. inches] of water. The four sides were firmly united by iron screws. The bottom was closed by a block of oak 3 dec. [11·8 inches] long, one end of which was rabetted to receive the sides, and these were fastened to it likewise by screws. The whole was covered with a cement impenetrable to water. On the bottom, namely in the end of the block, and perpendicular to its fibres, was fixed a small steel anvil of 3 cent. 25 mil. On two opposite sides, level with the anvil, were two leather boxes, by means of which a wire could traverse the box, without letting out the water. This wire, part of which rested on the anvil, was subjected to strong compression by means of a prismatic stamper of steel, which descended through the cover of the little box, and moved in a groove, that directed its lower extremity to the anvil. This stamper, driven by hard strokes with a hammer, transmitted to the wire, on which it rested, the blows it received, and compressed it. The hammer I employed weighed 2 kil. [4 lb. 6¾ oz.] At every stroke the wire was advanced two thirds of the breadth of the anvil. The metal subjected to experiment was an iron wire one third of a line in diameter, and of the weight of 366 grains [300 grs Eng.]. Compressed throughout by the mechanism I have described, it formed a band a little more than a line broad, and its length was increased; but its specific gravity had acquired no perceptible increase. The temperature of the water employed to collect the caloric evolved by this condensation rose only 2° [3·6°]. As my balance detects a variation of less than half a grain, a change in the density, less than would produce a difference of half a grain in the specific gravity of 366 grains of iron wire weighed in water, extricated a quantity of heat that raised the temperature of a cubic decimeter of water one degree, and consequently would have melted more than an eighteenth part of ice.

Friction produces an astonishing quantity of heat,

If the experiments I have described had not all the success I expected to determine the cause, that produces the heat extricated in the friction of substances, they appear to me not destitute of utility. They not only confirm the experiments of count Rumford concerning the astonishing quantity of heat produced by friction, but they prove (allowing for the irregularities unavoidable in experiments of this sort), that this quantity of heat is modified by the nature of the substances rubbed; that it is not in the ratio of the surfaces, for equal surfaces have given unequal quantities; and that it is not in the ratio of the number of particles rubbed off, or of the density; for lead, the density of which

and this is neither in the ratio of the surface rubbed, the density of the substance, nor the quantity abraded.

which is greater than that of copper, gave but an equal quantity of heat, while zinc, which is less dense than either, gave a greater quantity.

Influence of pressure,

The influence of pressure is shown by the 5th and 6th experiments, and as its effect on compressible bodies is necessarily to crush and condense them, does it not seem to indicate the approach of the particles as the cause of the extrication of heat? Yet as this condensation must be the greater, in proportion as the same pressure is exerted on a small number of points at a time, and is employed to detach particles, that cannot be separated till they have experienced a considerable approximation of their parts, the 7th experiment, in which the steel file-cut rubber detached a considerable quantity of particles of copper, should have produced a proportionate quantity of heat: yet it gave less by half than the friction with a smooth rubber.

and of condensation.

The influence of condensation on the production of heat is rendered still more uncertain by the 10th experiment, in which the iron wire, compressed and flattened by strokes of a hammer, ought to have set free a quantity of heat so much greater, in proportion as the force employed to effect the compression was more powerful. In vain would it be objected, that there was no condensation, the specific gravity of the wire not appearing to be changed: the electricity and fragility it had acquired, certain signs of compression, leave no doubt of the reality of this condensation, though it was too little to affect the hydrostatic balance. The condensation was greater than that produced by simple pressure, yet the quantity of heat extricated was less. These facts, the reverse of what might have been expected, render the explanation of the calorific phenomena produced by friction very difficult. They would seem to favour the opinion of count Rumford; but the materiality of heat is confirmed by so many arguments, that we ought not to relinquish a theory so fertile in useful explanations, before fresh experiments have completely elucidated this important point.

Difficulties in the hypothesis of material caloric,

In admitting, that the calorific phenomena, produced by friction, depend on the extrication of caloric, expelled from the pores of substances by forcing the particles nearer together, how is it that the particles, in resuming their former situation,

tuation, which necessarily takes place in elastic metals like zinc, do not resume the quantity of heat they have imparted to the water? and if this hypothesis explain the little heat of the 10th experiment, how does it apply to the great quantity produced by the simple pressure in the first, second, and third experiments? In discussing these facts, we are led to the following consequences. If the calorific phenomena produced by friction depend on a particular fluid called into action by this process, either this fluid is extricated from the pores of the metal by condensation, or it is drawn off and taken away from the surrounding bodies like the electric fluid. In the first case, the heat must diminish by condensation, must follow the inverse ratio of the density, and must be exhausted; in the second it must be modified by the insulation of the substances rubbed: which did not take place either in my experiments, or in those of count Rumford. If on the contrary these phenomena are produced solely by the internal agitation of the particles, the quantity of heat should diminish by condensation, and exhibit some proportion to the density and more particularly to the elasticity of the metal. Such are the doubts, that still obscure the question concerning the cause of the heat produced by friction, and require farther experiments. It is sufficient for me to have opened the way, and shown its importance and its difficulties.

and in that of vibrations of the particles.

VIII.

Abstract of a Paper read to the Institute the 21st of July, 1807, by Messrs. FOURCROY *and* VAUQUELIN, *on some Bones found in a Tomb in the Church of St. Geneviva*[*].

THESE bones were in general extremely fragile; but their heads were particularly so, owing to their great porosity.

Bones very tender,

Their colour is purple, much resembling that of dried wine

purple,

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[*] Annales de Chimie, Vol. LXIV, p. 190.

wine lees. This colour is much stronger in the body of the bones, than in their head, where on the contrary it is brownish.

filled with crystals,

In the body of the bones, as well as in their heads, are a number of white shining crystals, which have the appearance of sulphate of lime. These crystals, by forming in the interior of the bones, have raised their laminæ, and rendered them so brittle.

700 years old.

They are supposed to have been buried in the eleventh century, consequently to be at least 700 years old.

Boiled in water, rendered it acid,

Exp. 1. Being reduced to powder, and boiled in 300 parts of distilled water, they imparted to it a very pleasing red colour. The decoction was slightly acid. On the addition of ammonia its red colour was immediately destroyed, and a greenish precipitate formed, which became blueish in drying. The base of this precipitate was ammoniaco-magnesian phosphate.

lost 0·35, and much colour.

The substance of the bones thus boiled had lost only 35 hundredths of its weight. Its purple colour was exceedingly diminished.

Residuum dissolved in nitric acid.

The portion not dissolved by the water was in great measure dissolved in weak nitric acid witout any effervescence. Nothing remained but a few hundredths of a white powder, which was mingled with some brown membranes. This residuum will be noticed hereafter.

Phosphate of lime.

What the nitric acid had dissolved was phosphate of lime, mixed with a small quantity of red colouring matter.

The bone treated with nitric acid.

Residuum dissolved in alcohol gave out a red colour like archil.

Exp. 2. A piece of one of these bones immersed in weak nitric acid was soon dissolved, leaving only a soft red substance, which retained nearly the bulk and shape of the fragment. The nitric acid itself was turned red. The substance mentioned dissolved in alcohol, and imparted to it a very fine red colour, perfectly resembling that of archil dissolved in the same menstruum. When the substance in question was dissolved in alcohol, some brown flocks were left, being the remains of the membrane of the bone, that had escaped decomposition.

Colouring matter.

The matter that gives the purple colour to the bones therefore is soluble in alcohol, and even in water. Alkalis mixed with it give it a very fine green colour, perfectly similar to the tint observed in some decayed timber. This last mentioned substance, being dissolved in alcohol, likewise communicates to it a deep purple colour; but there is this differerence, it becomes green again on the addition of an acid, while that of bones, which is turned green by alkalis, has its red colour restored by acids.

Membrane.

The matter remaining after the action of alcohol on this red colour, that is to say the membranes, being subjected to the action of heat in a platina crucible, emitted a fetid vapour, then burned, and left only a few grains of sand.

Colouring matter supposed to arise from decomposition of the animal substance.

Without pretending absolutely to determine the origin of this red matter, Messrs. Fourcroy and Vauquelin are of opinion, that it proceeds from the decomposition of the animal substance. In fact we perceive many organized substances produce by their spontaneous decomposition, or putrefaction, colours that did not exist in them before. They themselves have described a colour, developed by the putrid decomposition of the gluten of wheat, which appears very similar to that of these ancient bones.

The crystals were phosphate of lime and of magnesia.

Exp. 3. As to the white shining crystals mentioned above, and found both on the surface of the bones and between their laminæ, Messrs. Fourcroy and Vauquelin satistisfied themselves, that they were formed of lime, phosphoric acid, and a little magnesia; and consequently that they were phosphates of lime and magnesia. The lamellar structure of these crystals, their brilliancy, and their flexibility between the teeth, had led those chemists at first to suspect, that they were sulphate of lime; though they were puzzled to explain the origin of the sulphuric acid. But having separated with great care a gramme of these crystals, they put them into diluted nitric acid, which dissolved them speedily and easily: and this solution was not precipitated by nitrate of barytes, which must inevitably have happened, had the crystals been sulphate of lime; but a copious precipitate was thrown down by oxalate of ammonia, and by ammonia alone. This salt fuses before the blowpipe much more easily than sulphate of lime; it diffuses, when kept long in fusion, a phosphoric light; and forms a semitransparent glass, which sulphate of lime does not.

To account for

Thoroughly convinced by these experiments, that the matter their solubility in water,

they were boiled,

precipitated by alkalis,

ter in question was composed of phosphoric acid, lime, and a little magnesia, it remained for Messrs. Fourcroy and Vauquelin to find how this compound could dissolve so copiously in water. With this view they boiled a gramme a long while, and several times in succession, in 400 parts of water. The first boiling had a rosy hue, evidently reddened paper tinged with litmus, and was copiously precipitated by alkalis, lime, and other alkaline substances. These precipitates had all the properties of neutral phosphate of lime.

and the fluid evaporated,

which left phosphoric acid.

After having precipitated by ammonia a pretty large quantity of the solution of this substance in water, they evaporated the fluid to dryness. During the evaporation some ammonia was given out. The matter that remained had a brown colour, attracted the moisture of the air, was acid, and formed a copious precipitate with limewater; which proved, that it was phosphoric acid.

Consequently superphosphate of lime with a little magnesia.

It is evident from the experiments related, as well as from several others, that the white crystalline substance, which covers and penetrates the bones found in the church of St. Geneviva throughout their whole extent, is a true acidulous phosphate of lime, containing a small quantity of phosphate of magnesia.

Whence comes the surplus of phosphoric acid?

From a new acid forming, and taking away part of the lime?

or from the decomposition of phosphorus contained in the animal matter?

But how shall we account for the formation of the phosphoric acid? We know, that this acid does not predominate in bones either dry or recent; and that, on the contrary, it is always accompanied witha a certain quantity of carbonate of lime. Messrs. Fourcroy and Vauquelin see no way of explaining this singular phenomenon, but by admitting, either that an acid is formed by the decomposition of the animal matter, which acid not only saturates the carbonate of lime, but takes away a portion of lime from the phosphoric; or that phosphorus existed in the animal matter, which has been converted into phosphoric acid, and afterward combined with the phosphate of lime, forming a superphosphate.

The latter more probable.

The latter appears to these gentlemen more natural, since it is more agreeable to the laws of affinity, and particularly to the discovery of phosphorus in animal substances. In fact no acid could have been formed by the decomposition of the animal matter but vinegar, or some other weak animal acid: but these acids could not take lime from the phosphoric; and besides, as we find nothing of this acid, or of the lime wanting to the bones, we must suppose, that they had been volatilized together, which is improbable.

In this case a large quantity of phosphoric acid formed.

If this supposition be as true, as it appears likely, a pretty large quantity of phosphoric acid must have been formed, since it was sufficient to saturate the carbonate of lime, which no doubt existed in the bones of the ancients, as it does in those of the moderns, and to convert part of the phosphate of lime into an acidule. The existence of the acidulous phosphate of lime in nature is not a new fact, for Messrs. Fourcroy and Vauquelin have sometimes met with intestinal calculi of herbivorous animals in this state, and exhibiting a decided crystallization: but they say, they never saw any, in which the acidity was so striking, or which were consequently so soluble in water.

Superphosphate of lime in intestinal calculi.

Remarks.

The examination of these bones informs us of several things, which appear to merit some attention. The first is the formation of a certain quantity of phosphoric acid by the decomposition of the animal matter, in which the phosphorus was indubitably[*] contained; the second, that this animal matter, by some change not perfectly known, gave rise to a very fine red colour, which is turned green by alkalis; the third, that this colouring matter had been preserved for so many ages[†], without being destroyed, which appears to have been owing to its combination with the phosphate of lime.

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[*] Just now the existence of the phosphorus in the animal matter was only presumed, as the most probable way of accounting for the existence of the superphosphate of lime.

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[†] This is a gratuitous supposition. If it did not exist originally in the bones, which is not supposed, who can say, how rapid or how slow the process was by which it was formed? and if a series of decompositions took place, it must evidently have been the result of the last of these.

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IX.

IX.

Experiments on the Tartarous Acid, and particularly on the Acid it affords by Distillation in the dry Way: by Messrs. FOURCROY *and* VAUQUELIN[*].

Pyroligneous & pyromucous acids found to be compounds of the acetic, and the pyrotartarous supposed to be the same,

MESSRS. Fourcroy and Vauquelin having examined the pyroligneous and pyromucous acids in the year 8, found that they were formed of acetic acid and an empyreumatic oil. Having afterward examined the pyrotartarous acid, they perceived, that it was volatile; and that with potash it composed a salt in foliated crystals with the appearance of mother of pearl, attracting moisture from the air, having a pungent and acid taste, totally soluble in alcohol, and emitting a pungent acid smell when acted on by sulphuric acid. From these experiments they inferred, that tartar afforded by distillation the same acid as gum, starch, wood, &c.: and in fact it was scarcely possible, to form any other conclusion; for beside the properties of the pyrotartrite of potash, which are nearly the same as those of the acetate, their opinion was supported by analogy.

but this questioned.

But Mr. Gehlen having said in a letter printed in the Annales de Chimie for October 1806, that he could not believe the pyrotartarous acid to be the acetic, because, after slow evaporation it left a crystallized residuum, which differed too from tartarous acid, Messrs. Fourcroy and Vauquelin have examined the pyrotartarous acid anew, and the following is the result of their inquiries.

Pyrotartarous acid combined with potash.

1. The acid liquor obtained by the distillation of tartar being saturated with carbonate of potash, a part of the oil dissolved by this acid was precipitated in the form of a brown resin, yet a large quantity remained in the new compound.

The salt crystallized.

2. This compound, evaporated to dryness and redissolved several times in water, yielded a salt of a brownish colour,

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[*] Annales de Chimie, vol. LXIV, p. 42.

a hot and pungent taste, and a foliated form, like acetate of potash.

Precipitates acetate of lead.

3. This salt precipitated the nitrates of mercury and of silver in white scales; but it precipitated the solution of acetate of lead also, which acetate of potash does not.

4. Exposed to the fire it swelled up, and was carbonized.

Distilled with sulphuric acid.

5. Distilled with diluted sulphuric acid by a gentle heat, it turned black, and toward the end of the process yielded a white sublimate, which adhered to the whole surface of the retort in the form of scales. The liquid, that came over before the sublimate appeared, had a very decided acidity, which was not owing to the sulphuric acid employed; but it had only a very slight smell of vinegar.

Singular phenomenon.

6. This process, the distillation of the salt formed by the pyrotartarous acid and potash, afforded a singular appearance. The acid liquid just mentioned contained at the bottom a large globule of another liquid, with a slight yellow tinge, that rolled about when the vessel was moved without mixing with the liquid containing it. It resembled phosphorus melted at the bottom of water. As it was night, Messrs. Fourcroy and Vauquelin stopped the vessel very close, in order to examine it more accurately the next day: but in twelve hours it was not to be seen, the heavy globule having mixed with the other liquid during the night.

Crystals produced.

7. The retort being cut, the crystals were separated as completely as possible, and exhibited the following properties.

Their properties.

a. Their taste was extremely acid.

b. They readily melted, and evaporated in white fumes, when placed on a heated substance.

c. They dissolved copiously in water, and crystallized again by spontaneous evaporation.

d. Their solution did not precipitate that of acetate of lead, or that of nitrate of silver, but it precipitated nitrate of mercury. However, some time after this acid had been mingled with a solution of acetate of lead, needly crystals were found it arranged in plumes.

e. The solution of this acid partly saturated with potash does not furnish an acidule as the tartarous acid does, but it immediately precipitates acetate of lead, though the sublimed concrete acid does not, when employed singly and pure.

f. The neutral compound of this acid with potash, in a state of deliquescence, is soluble in alcohol. It does not precipitate the salts of barytes or of lime, as the alkaline tartrites do.

g. The liquid obtained in the same process as the crystals just mentioned, being evaporated with a very gentle heat, likewise furnishes crystals, which have properties precisely similar to the former.

The pyrotartarous a distinct acid.

From these experiments it follows, that the pyrotartarous acid is a peculiar acid: that it differs from the acetous in being less volatile, in having less smell, in crystallizing by evaporation, and in its combination with potash precipitating acetate of lead: that it differs from tartarous acid in not precipitating lime, barytes, or acetate of lead: and that it does not form with potash an acidule of difficult solubility.

Distinguished from the acetic.

Messrs. Fourcroy and Vauquelin made one experiment, which incontestably shows, that the pyrotartarous acid is not acetic acid changed in its properties by combining with the oil produced at the same time. They repeatedly distilled concentrated acetic acid with the oil procured by the dry distillation of tartar. They then combined this acid with potash, distilled the salt thus produced with sulphuric acid, and obtained nothing but empyreumatic vinegar.

Some acetous acid formed.

It appears however, that a small quantity of acetous acid is formed in the distillation of tartar, from the pungent smell emitted when sulphuric acid is poured on the salt formed by combining the empyreumatic acid with potash.

Pyromucous & pyroligneous compounds of the acetic.

The pyromucous and pyroligneous acids, being subjected to fresh experiments, comported themselves like acetic acid holding in solution empyreumatic oil.

So is the formic.

The authors have examined anew the acid of ants, which Mr. Gehlen says is different from vinegar; and they have confirmed the identity of these two compounds, at the same time showing, that the former contains a little phosphoric acid combined with an animal matter.

Analysis of cream of tartar.

They conclude their paper with an analysis of tartar, or the supertartrite of potash. They found, that 1000 parts of this salt, of the finest quality, yielded by distillation, exclusive of the acid product and coal,

Pure dry carbonate of potash, ····	350 parts
Tartrite of lime ················	6
Silex ························	1·2
Alumine ······················	0·25
Iron mingled with manganese ····	0·75

X.

On an Improvement in the Manner of dividing Astronomical Instruments. *By* Henry Cavendish, *Esq. F. R. S.*[*]

Inconvenience of the common method of dividing instruments.

THE great inconvenience and difficulty in the common method of dividing arise from the danger of bruising the divisions by putting the point of the compass into them, and from the difficulty of placing that point midway, between two scratches very near together, without its slipping towards one of them; and it is this imperfection in the common process, which appears to have deterred Mr. Troughton from using it, and thereby gave rise to the ingenious method of dividing described in the preceding part of this volume †. This induced me to consider, whether the above-mentioned inconvenience might not be removed, by using a beam compass with only one point, and a microscope instead of the other; and I find, that in the following manner of proceeding, we have no need of ever setting the point of the compass into a division, and consequently, that the great objection to the old method of dividing is entirely removed.

Method of remedying this.

Apparatus for dividing astronomical instruments described.

In this method it is necessary to have a convenient support for the beam compass: and the following seems to me to be as convenient as any. Let C C C (Plate I, Fig. 1,) be the circle to be divided, B B B a frame resting steadily on its face, and made to slide round on it with an adjusting motion to bring it to any required point: $d\delta$ is the beam compass,

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[*] Philos. Trans. for 1809, p. 221.

† See Journal, vol. XXV, p. 1, and 100.

pass, having a point near δ, and a microscope *m* made to slide from one end to the other. This beam compass is supported at *d*, in such manner as to turn round on this point as a centre, without shake or tottering; and at the end δ it rests on another support, which can readily be lowered, so as either to let the point rest on the circle, or to prevent its touching it. It must be observed, however, that as the distance of *d* from the centre of the circle must be varied, according to the magnitude of the arch to be divided, the piece on which *d* is supported had best be made to slide nearer to, or farther from, the centre; but the frame must be made to bear constantly against the edge of the circle to be divided, so that the distance of *d* from the centre of this circle shall not alter by sliding the frame.

Method of using it for division by continued bisection.

This being premised, we will first consider the manner of dividing by continued bisection. Let F and *f* be two points on this limb, which are to be bisected in φ. Take the distance of the microscope from the point nearly equal to the chord of *f* φ, and place *d* so that the point and the axis of the microscope shall both be in the circle, in which the divisions are to be cut. Then slide the frame B B B, till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the centre *d* till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point *f*, make another faint scratch with the point; which, if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point midway between them will be the accurate bisection of the arch F *f*; but it is unnecessary, and better not to attempt, to place a point between these two scratches.

Having by these means determined the bisection at φ, we must bisect the arches F φ and *f* φ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

It must be observed, that, when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise

Method of using it for division by continued bisection.

it could not be brought near enough to the axis of the microscope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. Troughton's expedient of bisecting an odd number of contiguous divisions.

It must be observed too, that, in the bisections of all the arches of the same magnitude, the position of the point *d* on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support *d* must be placed so that *d* δ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

It is plain that in this way, without some farther precaution, we must depend on the microscope not altering its position in respect of the point during the operation; for which reason I should prefer placing the axis of the microscope at exactly the same distance from the centre of motion *d*, as the point; but removed from it sideways, by nearly the semidiameter of the object glass; so that having made the division, we may move the beam compass till the division comes within the field of the microscope, and then see whether it is bisected by the wire, and consequently see whether the microscope has altered its place.

In the operation of bisection, as above described, it may be observed, that, if the two scratches are placed so near together, that in making the second the point of the compass runs into the burr raised by the first, there seems to be some danger, that the point may be a little deflected from its true course; though in Bird's account of his method, I do not find that he apprehends any inconvenience from it. One way of obviating this inconvenience, if it does exist, would

Method of using it for division by continued bisection.

would be to set the beam compass not so exactly to the true length, as that one scratch would run into the bur of the other; but as this would make it more difficult to judge of the true point of bisection, perhaps it might be better to make one scratch extend from the circle towards the centre, and the other from it.

It is clear, that the entire arc of a circle cannot be divided to degrees, without trisection and quinquesection; and I do not know whether our artists have recourse to this operation, or whether they avoid it by some contrivance similar to Bird's, namely, that of laying down an arch capable of continued bisection; but if the method of quinquesection is preferred, it may be performed by either of the three following methods:

Method of dividing by quinquesection.

First method. Let a α (Fig. 2) be the arch to be quinquesected. Open the beam compass to the chord of one fifth of this arch; bring the microscope to a, and with the point make the scratch f; then bring the microscope to f, and draw the scratch e; and in the same manner make the scratches d and b. Then turn the beam compass half round, and having brought the microscope to α, make the scratch β; and, proceeding as before, make the scratches δ, ε, and φ. Then the true position of the first quinquesection will be between b and β, distant from β by one fifth of $b\,\beta$; and the second will be distant from δ by two fifths of $d\,\delta$, and so on. Then, in subdividing these arches, and striking the true divisions, the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to b. But in order to avoid the confusion, which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, showing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye may be able to place the wire in the proper manner, between the two scratches, without farther assistance; but the most accurate way would be to have a movable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to *b*, bring the movable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the movable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to *b*.

It will be a great convenience, that the movable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable errour in the distance of the movable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

To judge of the comparative accuracy of this method with that of bisection, it must be considered, that the arches $\alpha\beta$, $\beta\delta$, &c., though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch *a* α into five parts, the beam compass is opened to the exact length, but that from the abovementioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\epsilon$, and $\epsilon\varphi$ are all too long by the small quantity ϵ, and that the arches *af*, *fe*, *ed*, and *db* are all too short by the same quantity, which is the supposition the most unfavourable of any to the exactness of the operation; then the errour in the position of $\beta = \epsilon$, and the point *b* errs 4ϵ in the same direction, and therefore the point assumed as the true point of quinquesection will be at the distance of $\frac{3\epsilon}{5}$ from β, and the errour in the position of this point $= \epsilon \times 1\frac{3}{5}$.

This method compared with that by bisection.

By the same way of reasoning the errour in the position of the point taken between *d* and $\delta = \epsilon \times 2\frac{2}{5}$.

In trisecting the errour of each point $= \epsilon \times 1\frac{1}{3}$; and in bisecting the errour $= \epsilon$; and in quadrisecting the errour of the middle point $= 2\,\epsilon$.

It appears therefore, that in trisecting, the greatest errour we are liable to does not exceed that of bisection in a greater proportion than that of 4 to 3; but in quinquesecting the errour of the two middle points is $2\frac{2}{5}$ times greater than in bisecting. It must be considered, however, that in the method of continued bisection, the two opposite points must be found by quadrisection; and the errour of quinquesection exceeds that of quadrisection in no greater proportion than that of six to five; so that we may fairly say, that if we begin with quinquesection, this method of dividing is not greatly inferior, in point of accuracy, to that by continued bisection.

2d method of dividing by quinquesection.

Second method. This differs from the foregoing, in placing dots or scratches in the true points of quinquesection and trisection, before we begin to subdivide. For this purpose, we must have a microscope placed as in page 49, fourth par., at the same distance from the centre of motion as the point is; and this microscope must be furnished with a movable wire and micrometer, as in page 51; and then having first made the fixed wire of this microscope correspond exactly with the point, we must draw the scratches *b* and β, *d* and δ, &c., as before, and bring the fixed wire to the true point of quinquesection between *b* and β, in the manner directed in page 226, and with the point strike the scratch or dot; and if we please we may, for farther security, as soon as this is done, examine, by means of the movable wire, whether this intermediate scratch or dot is well placed.

Advantage of this method.

The advantage of this method is, that when this is done, we may subdivide and cut the true divisions, by making the wire of the microscope bisect the intermediate scratches, instead of being obliged to use the more troublesome operation of placing it in the proper proportion of distance between the two extremes.

Its disadvantage.

This method certainly requires less attention than the former, and the whole seems to be attended with considerably less trouble; but it is not quite so exact, as we are liable to the double errour of placing the intermediate point, and of subdividing it.

As in this method the intermediate points are placed by means of the micrometer, there is no inconvenience in placing the extreme scratches b and β, &c., at such a distance from each other, that the intermediate one shall be in no danger of running into the bur raised by the extremes.

3d method of dividing by quinquesection.

Third method. Let $a\,\alpha$ (Fig. 3) be the arch to be quinquesected; lay down the arches $a\,b$, $b\,d$, and $d\,e$, as in the first method; then turn the beam compass half round, and lay down the arches $\alpha\,\beta$ and $\beta\,\delta$; then, without altering the frame, move the movable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered; but as this method possesses not much, if any advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying any thing farther about it.

Farther general observations.

It was before said, p. 48, that the centre of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let A δ (Fig. 4) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the centre on which it turns, and M m the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose, that this point is not exactly in the line $d\,\delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much one side of $d\,\delta$, as m is on the other, so that if the wire is not perpendicular to $d\,\delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason care should be taken to make the wire exactly perpendicular to $d\,\delta$, which is easily examined by observing whether a point appears to run along it, while the beam compass is turned a little on its centre. It is also necessary to take care, that the point δ is in the arc of the circle, while the bisection is observed by the microscope, which may most conveniently be obtained, by placing a stop on the support on which that end of the beam compass rests. If proper care, however, is taken in placing the wire perpendicular, no great nicety is required either in this or in the position of *d*.

Another thing to be attended to, in making the wire bisect two scratches, is to take care that it bisects them in the part where they cut the circle; for as the wire is not perpendicular to the circle, except in very small arches, it is plain, that if it bisects the scratches at the circle, it will not bisect them at a distance from it.

There are many particulars in which my description of the apparatus to be employed will appear incomplete; but as there is nothing in it which seems attended with difficulty, I thought it best not to enter farther into particulars, than was necessary to explain the principle, and to leave the rest to any artist who may choose to try it.

Mr. Troughton's method.

It is difficult to form a proper judgment of the conveniencies or inconveniences of this method, without experience; but, as far as I can judge, it must have much advantage, both in point of accuracy and ease, over that of dividing by the common beam compasses; but it very likely may be thought, that Mr. Troughton's method is better than either. Whether it is or is not, must be left for determination to experience and the judgment of artists. Thus much, however may be observed, that this, as well as his, is free from the difficulty and inaccuracy of setting the point of a compass exactly in the centre of a division. It also requires much less apparatus than his, and is free from any danger of errour, from the slipping or irregularity in the motion of a roller; in which respect this method, notwithstanding the precaution used by him, is perhaps not entirely free from objection; and what with some artists may be thought a considerable advantage, it is free from the danger of mistakes in computing a table of errours, and in adjusting a sector according to the numbers of that table.

XI.

XI.

Description of a Machine for securing Persons attempting Depredations, without affecting their Lives or Limbs. By Mr. ROBERT SALMON, *of Woburn**.

SIR,

Man-traps useful to check petty thieves,

I Beg leave to submit to the Society of Arts &c. a Man-trap, which I hope will meet with their approbation. To those who live in the country it is needless to explain the frequency of petty depredations committed on gardens, orchards, &c., which are sometimes very vexatious. Few persons would like to endanger the life or limb of the depredator by setting the common steel man-trap; yet it is presumed there are but few, who would not wish to detect the offender. The instrument which I have the honour to submit to the Society is for the purpose of catching and holding the person without injury.

but the common ones dangerous.

Contrivance to secure a thief without injuring him

At the agricultural meeting at Woburn last summer an ingenious invention for a similar purpose was produced by Sir Theophilus Biddulph; it consisted of a wooden box, containing two springs in iron barrels, and two chains passing over and round them; when this was set, the chains were withdrawn from round the barrels, and extended to a certain distance. A trigger then kept the trap from closing, the whole was then covered over with thin iron plates, so that if a person set his foot on these plates his leg dropped into the box, and the chains closed round it and held the leg; but as the box was about three feet square and a foot deep, it was requisite that it should at setting be let into the ground, which would be a work of considerable labour; and when done it would be difficult to dispose of the stuff from the hole, or to conceal the trap; and as the whole apparatus was cumbersome and expensive, it appeared to me not to be well applicable in practice.

too cumbersome.

I think

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* Trans of the Soc. Arts, vol, XXVII, p. 181. The silver medal of the Society was voted to Mr. Salmon for this invention.

Another simple contrivance for the purpose.

I think it right to give this explanation in justice to Sir Theophilus Biddulph, from whom my idea of the utility of something of the kind arose, as also to show the difference between his invention and the trap I have made, which is so very simple as hardly to require explanation. When set it only requires, that the two keys be withdrawn, and that the trap be covered with a few loose leaves or mould. To the trap I have attached a piece of chain and a screw to be screwed into the ground, so as to prevent its being carried away; against any person that may be caught such a precaution is perhaps unnecessary, for any person who is caught will find the jaws of the trap close so fast on the leg, that he cannot drag the trap far without great pain, and will consequently be glad to stand still, and to call out for relief. For the convenience of explanation I have applied mufflers to the jaws of the trap, so that any person may put in his leg without the least inconvenience. I have even tried it without, yet, though void of danger, the sensation is not pleasant. The muffler will of course be omitted when set for use, as it is not then necessary to guard against a little inconvenience, otherwise the springs might be made weaker.

I remain, Sir,

Your most obedient humble servant,

ROBERT SALMON.

A screw contrived for fixing any thing steadily in the ground

P. S. Permit me strongly to recommend to the notice of the Society the earth-screw attached to the trap, as excellent for the purpose of fixing any thing steadily in the earth. This screw is far superior to the common way of driving an iron point or stake therein.

very useful on various occasions.

I have employed it for several years in fixing cross-staves and other surveying instruments with great advantage. The very act of driving a spiked instrument into the earth leaves it loose with some play or movement, which prevents it from being easily secured; but with a screw of this kind at the bottom of the instrument it is firmly fixed in the ground, and a turn of the screw will again fix it, if it should by any means be moved or loosened. It may also be screwed into the ground with any instrument upon it, which would be spoiled by the act of driving it in.

Description

Description of Mr. Salmon's Man-trap, which detains the Offender, without injuring or maiming him. See Plate II, Fig. 1.

Description of the trap.

The principal figure in the fore-ground of Plate II is a perspective view of this machine. Fig. 1, A B C is a frame of wrought iron, about 18 inches square; it has an eye projecting from it to receive a short chain, the other end of which is fastened to an iron screw, shown separately at D, screwed into the earth by the key or handle E; this screw is about 14 inches long, and, when screwed into hard ground, will hold so firmly, that there is no danger of its being drawn out, even by two or three men, and having a small square end, it cannot be turned without the key or handle E; so that an offender would find it extremely difficult to remove the trap: *e f e g* are two iron frames moving on centres in the frame A B C; these frames have a constant tendency to close together, by means of two springs, *p p*, fixed in the frame A B, and acting against pins projecting from the upright sides of the moveable frame *e e*; *k k* are two small iron rods jointed to the upper rod of the movable frame *g*, and passing through small locks, *l l*, fixed to the other frame *f*. These locks contain clicks which are pressed by springs into the teeth, as may be seen upon the rods *k k*, so as to prevent the two bars *f g* from being drawn asunder when they have been closed by means of the springs *p p*. The internal mechanism of the locks is explained by figures 2, 3, on a larger scale at L M, in the same plate; one side of the lock is supposed to be removed to exhibit its interior parts, where *k* represents the rack, or that part of the rod which is cut into teeth, *r* is the click, which engages the teeth of the rack, and prevents its being drawn through the lock: the click is pressed against the teeth of the rack by a spring, which is plainly seen in the figures; the locks are attached to the ends of the bar *f* of the movable frame, by the bar passing through the locks, and when the lids are rivetted on it is confined in such a manner that it cannot be got out. But as it is necessary to open the bars *f g*, and draw the clicks back from the teeth of the racks, Mr. Salmon has contrived two different methods of accomplishing this object. Figure 3, M, is that which is used in the model left at the Society's Repository. A small key or screw S is put down through a hole in the lid of the lock, and is received into a hole lapped with a screw in the click; by turning the screw it lifts the click out of the teeth of the rack; so that the moving frames *f g* can be opened apart from each other, till they lie flat upon the frame A B. The iron cross *m* is then put between the two rods *f g*, the screws S of the two locks are to be withdrawn from the locks, and the trap is set for use. If an offender should place his foot within the square of the frame, he would tread down the cross *m*, and having thus removed the obstruction, the two frames *e f e g* are closed together by the springs *p p*, so that the bars *f g* enclose his leg, and the clicks in the locks prevent the bars being opened without the screws S. In some of the machines which Mr. Salmon has made since the model was deposited with the Society, the locks are made like figure 2, L, where a common key is to be introduced, and, when turned round, catches the tail of the click; it may have wards to prevent the using of a false key, though no wards are shown in the plate.

Screw for fixing any thing in the ground.

Part of the screw D for securing the trap from being carried away by depredators, is shown on a larger scale at N, in order that the peculiar form of its threads may be better seen, which fix it firmly in the earth. Such screws would be very serviceable in fastening horses at grass, &c.

XII.

Method of constructing commodious Houses with Earthen Walls. By Mr. ROBERT SALMON, *of Woburn*[*].

DEAR SIR,

Building houses with earth.

HAVING for some years past practised at this place the art of pisé, or constructing walls with earth; and having in consequence been several times, both publicly and privately, called on to communicate my observations thereon; I have been led to consider, that the best mode of generally communicating what I know on the subject would be through the medium of the Society of Arts &c. I have accordingly, by the waggon, forwarded a case containing a model of my frames and apparatus for performing the work, with every particular in my power to give, for the information of any persons inclined to build in that way; and they will, I hope, be found worthy a place in the collection of the Society.

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[*] Trans. of the Soc. Arts, vol. XXVII, p. 185. Twenty guineas were voted to Mr. Salmon for this communication.

Some near London.

To such as may be inclined to see specimens of this work, and may not have an opportunity of going far distant from London, I can recommend a house and other works built, and some of them inhabited by my brother, Mr. William Salmon, Builder, at Henley Hill, near Barnet, Herts.

I have the honour to be,

The Society's and your most obedient servant,

ROBERT SALMON.

Description of the Engraving of Mr. Salmon's Method of building Pisè or Earthen Walls.

Explanation of the plate.

Fig. 4 of plate II is a perspective view of the apparatus or moulds, in which the earths are rammed to form a wall. The mould consists of two long planks F *f*, twelve feet long, twenty inches broad, and one inch thick, each made in two breadths; they are strengthened by several pieces of wood nailed across them. Holes are made through these pieces of wood at top and bottom, to receive iron bolts, which hold the two boards parallel to each other, fourteen or sixteen inches asunder, which is the thickness of the wall intended to be formed between them. The bolts have a large head at one end, and a key passes through the other, to keep the planks together. When a wall is to be built, the foundation is laid in brickwork, which is carried about nine inches above the ground, upon this brickwork the planks are placed and bolted together. Two boards, like that shown at G, are placed between the planks at the ends, to form the ends of the mould; these boards are placed between the two bolts

Explanation of the plate.

a a, which are seen close together at the end of the moulds, and are held fast by that means; the earth is now to be rammed in between the moulds, by the rammer with an iron head X. When the mould is filled with earth and well rammed down, the keys are to be taken out of the bolts, and the bolts drawn out; the planks are then removed, and put together again, a length farther upon the wall, the bolts at the end being put through the holes left in the wall. Only one of the end boards is now put in, and the ramming proceeds as before. In this manner straight walls may be built of any length, and when the lower course is finished, then the mould may be taken to pieces, and put together again upon that course; the lower bolts of the frame being put through the bolt holes, which the upper bolts made in the wall at the first operation, to insure that the upper part of the wall is in the same place, and exactly over the lower. When a wall is to be built thinner than usual, a block of wood must be placed under the head of each bolt, so as to diminish the space between the planks.

When the angle walls of buildings are to be made, the apparatus is put together, as shown in the plate; four of the planks are put together to form a right-angled mould; one end of each of the planks F and H is furnished with double bolts, the other ends have each two eyebolts fixed into them, as shown separately at *b d*; then a bolt *n* connects the two moulds, so as to form a hinge; the planks are kept together so as to be perpendicular to each other, by a long iron rod K, hooked into eyebolts fixed in the planks. The outside planks of the mould are joined together in a different manner, see fig. 5, that of one frame being longer than that of the other, and has two pair of holes through its end O, to receive the bolts *t t*, which are fastened to the ends of the other shorter plank, and the keys are put through the ends of the bolts, to secure the planks together; a piece of wood P is occasionally placed between the end of the short plank and the side of the other, to increase the space between the planks, to make a thicker wall, the two bolts at the end of the plank being received into the notches in the piece of wood, and these bolts are then put through the holes ZZ of the long plank. In building the angle wall, it is necessary, that the vertical joints formed between each mould should not be over one another, but arranged in the same manner as the joints of brickwork; this is accomplished by making the lower course of wall upon the brickwork only half the length of the mould, which is done by placing the end board G of the mould in the middle of it. The next course over this is to be made the whole length of the mould, the next one only half, and so on, as shown in the figure.

The model of the frame in possession of the Society is made to a scale of an inch to a foot. The frame at large is made of 1¼ inch deal, ploughed and tongued together; the bolts and pins or keys of iron, as are also the plates on the holes in the sides of the frame. These plates are put to prevent the keys from cutting into the wood, and the holes from gulling and wearing.

Method of building earthen walls.

This sort of mould is calculated for making walls either fourteen or sixteen inches thick, and the model (or perspective view of it in the distance of plate II) shows how the mould is to be applied for making the corner of a building of the sixteen inch wall; the same moulds may be applied for a fourteen inch wall, *ff* being the outer sides, F H the inner sides. When employed for straight walls, or making good between the corners of buildings, the two returns of the frames are used in pairs, *ff* and F H make two sets of frames. The board marked G must be of width equal to the thickness of the walls to be made, and is for the purpose of stopping the earth, and making ends or jambs to doors or windows, or wherever wanted. The piece of wood P is two inches thick, and is for the purpose of making out the external sides of the moulds, from a fourteen inch to a sixteen inch wall: by introducing this piece between the two sides *ff*, and putting the fixed iron pins in the outer holes Z Z, and taking away the blocks under the heads of the outer bolts, the sides of the frame will then be sixteen inches, as under, and thereby adapted for a sixteen inch wall. Fig. E are pieces of wood about 1¼ inch square, and cut to the length of the thickness of the wall, and are for gauges to be applied on the top of the bolt, to keep the keys from drawing the sides too close together.

Method of building earthen walls.

In beginning the wall, some of them are necessary at the bottom, the more firmly to support the frame on the brick or stone work. They are then worked into the wall, and, after the frame is taken down, driven out. After the first course, they are only necessary to the top irons, and may be taken out as soon as the earth is rammed up near them, so that no holes are left in the upper courses of the wall, more than the bolt holes.

When these frames are used, one side is placed in such a direction, that the front or end may be required to be taken away, and then by means of the angular iron brace K, the other return is sure to stand at right angles with the first. Care should then be taken, in the first course, to set the sides level, that being done, the other upper courses, from the nature of the frames, and manner of using them, must of course come upright and level without any particular care, and a wall properly begun cannot well get wrong. After the first course of a building is done, the moulds should be moved to another, and so on till all the courses are up, and as the top holes of each preceding course become the bottom holes in the succeeding ones, no difficulty will be found in fixing the mould after the first course is properly done.

Fig. 6 shows the iron pin and staples that keep the internal angle of the frame together. K, fig. 4, is an iron stay to set the returns at right angles. This is only wanted where other means of setting the building square is not to be obtained.

Having described the frame, and means of applying it generally, it may be necessary to observe the following particulars in the process. Having carried one course round the building, it frequently happens that the top thereof becomes too dry to attach to the next succeeding course; and therefore it is advisable, that, as soon as the frame is set for the succeeding course, a small quantity of thick grout, composed of ¼ lime, and ¾ earth, be poured on the top of each course, immediately before the first layer of earth is put in. A very small quantity is sufficient, and will add much to the strength of the work, by cementing the courses well together at the joints. The workman should also, with the corner of his rammer, in ramming home to the upright joints, cut down a little of that part of the wall, up to which he works; this will make the upright joints key together, and unite in a solid manner. Having thus proceeded and got up the walls, the next thing will be to stop the bolt holes, with mortar made ¼ lime and ¾ earth the same as the wall.

Earth proper for the purpose.

The earth proper for this work should be neither sand nor clay, but partaking of both. Clay is particularly objectionable, as is also chalk, or calcareous earth of any sort. Sand is also not proper, unless accompanied with some binding quality: the bolder and coarser the sort of earth the better. When used, it should retain no more moisture than just to make it adhere together, under the pressure of the thumb and finger. Notwithstanding earths bordering on sand appear to make the strongest work, nevertheless good earths may often be found in parts that do not abound with sand. Those that abound with a mixture of grit or fine gravel are generally the best. Having provided proper earth, as much should be put in each layer as to form about an inch and half when compressed by ramming.

The rammer.

The rammer X should not be more than half an inch wide on the edge, in order that it may more forcibly compress every part of the earth, which a flat rammer would not do so well.

Composition of the wall.

In making the walls, about three inches in thickness of loose earth should be put in each course, which done, the same, by means of a trowel made for the purpose, is drawn back and cleared from the face of the wall, and the space then filled up with the facing composition, forming on an average about one inch in thickness; the whole then is firmly rammed, (in which, and properly preparing the facing stuff, much depends the perfection of the work,) till it is quite hard, when it will be compressed to about one inch and a half in thickness. The common facing stuff is composed of lime one part, and earth, the same sort as used for walling, three parts. The lime and earth mixed and slacked together, the same as for mortar. The more it is slacked and wetted the better, provided time can be allowed for it again to dry and pulverize, so as to be fit for ramming. The better sort of facing stuff may have a small quantity more of lime in it.

Foundation.

The foundation should be of brick or stone, carried up nine inches above the ground; and if a plinth is to be shown, then one course above the same should be of brick or stone, to prevent the water that might lodge on the plinth from damaging the earth wall.

Season for building.

The proper season for performing this work is any time that the earth is to be procured sufficiently dry for the purpose; the more early in the season the better, in order to give it time to dry before finishing, or if late it would be advisable not to finish till the year after it is built.

Windows and doors.

Windows and doors may be left in the walls, wherever wanted, by fixing the head of the moulds and carrying up quoins to form the same: in erecting which some bond timber should be laid in course mortar and rammed in with the earth. Lintels may also be laid at the proper height. This method is cheapest, where only one window or door of a size is wanted, but if many, the readiest way would be to make some rough frames of boards of width equal to the thickness of the walls, and place them in the situation of the windows and doors. When done the earth is rammed up to them, laying bond timber at the sides and lintels over them. In both cases the windows and door frames are to be put in their places, and fastened to the bond timber, after the wall is up. The bond timber, lintel, and plates, should be kept as thin as possible, in order to prevent any disagreement between the earth and timber in the shrinking or drying of the same. The bond timber about 4 inches by 1½; floor or wall plates 6 inches by 2; lintels about 4 inches thick; and it may be worthy of notice, that any slabs or rough stuff may be used, the earth being sure to ram close to it and keep it in place.

Finishing of ordinary work.

For common cottages, when the whole of the walls are up and covered in, the holes should be stopped with very coarse mortar, made the same as the facing stuff, but used wetter, and the wall then lime-washed over with lime and sharp sand, which should be made up in small quantites and used while hot. This may readily be done by adding a knob of lime and sand a little at a time as it is used.

Finishing of better work.

For better kind of cottages the better sort of facing stuff may be used, and then, as before, the whole lime-whited; or if it be required to make the finishing as perfect as possible, the following is the best mode, viz. with water and a brush thoroughly wet and soak the face of the wall for two or three yards in superficies at a time; all which part, during the said wetting, should be continually rubbed and worked about with a hand float, till such time the face is rubbed smooth and even, by which the facing composition will so wash up as to become a pleasant regular colour, the face smooth and hard when dry, and not liable to scale off as a coat of plastering would do. This finishing will be still improved by a small quantity of lime being put into the water used for soaking the face: and if, after the wall is well soaked and rubbed, as abovementioned, there be thrown thereon with a brush some lime and sand, (such as is used for lime-whiting,) and this also worked into the face; the face will then become as perfect and hard as stucco.

Having explained the frames as constructed by me for performing earth walling, as also the manner of finishing it, I beg leave to lay before the Society some observations on these, compared with the original French means and manner of performing the same, as described in the first volume of Communications to the Board of Agriculture.

Original French mode.

The sides of the frames, as formerly constructed, were supported on joists or cross pieces of timber, which pieces were cut into the top of each course of walling. The sides were then kept together by upright timbers framed into the cross pieces or joists, and the tops of the upright pieces were wristed and held together by ropes going across the frame from one side to the other. In consequence of this construction, by experience I found much labour was lost in cutting the channels to lay the cross pieces in. These channels, after the buildings were up, took labour and materials to fill them in, and rendered the walls less strong. Also the difficulty of getting the frame rightly placed every time it was moved, and the elasticity of the rope across the top, made the whole very imperfect, so much so that all work done in that manner was untrue and unsound; as the rope, however tight it might be strained, would yield to a certain

Its inconveniences.

degree. The labour of moving was great, and when the frames were set, the cross ropes and uprights above the sides were much in the way of the workmen.

Advantages of the new mode.

On examining the model I have the honour to send, it may be seen, that these frames being once set true, they require very little care afterward: being kept together by iron bolts no elasticity can occur, and the earth will be as firmly compressed, as if rammed between two walls. No cutting away for cross pieces is required, nor any holes but the small bolt holes to make good; and as nothing sticks up above the frames, the workman cannot be impeded. In consequence of these alterations the work may be more cheaply and truly executed than with the old sort of frame.

Previously to entering into the expense of this sort of work, or my conceptions as to its advantage, it may be necessary briefly to state whence such are collected.

Trial of the old mode,

About sixteen or eighteen years ago, the late Duke of Bedford directed a foreigner, who was then making some walls in Lancashire, to come and make some specimens here; and wishing to know how far it might be usefully introduced, I was directed to give attention, and every aid, to the man employed. Accordingly frames of the old sort were made, exactly like those before described, and with them some specimens being made, the man returned. These specimens I considered were very bad walling, and in attending to the execution thereof, seeing sufficient room for improvement,

and of the new.

I was directed farther to practice it. Frames were then constructed like the model, and several walls erected, among which were some cottages now standing, and lastly, the house I now live in. This has been built about twelve years, and is a sufficient proof of the utility of the practice: the house being as close, warm, and dry in the walls, as if built of any materials whatever.

Expense.

With regard to the expense of the walls of this sort, as labour is the principal part of the expense, and as in some places labour is dearer than in others, the best mode of estimating it at different places will be from the quantity that a man should do in a day, and which I have found to be 1¼ yard superficial, in the common day's labour of ten hours.

At this place the expense may be estimated as follows:

	£	s.	d.
Labour to making facing composition, fitting in and ramming to a 16-inch wall, where the earth is at hand (labourer's wages being 1*s.* 10*d.* per day) per yard superficial	0	2	2
Value of lime used in the composition rammed into the face of a yard superficial (lime being 8*d.* per bushel)	0	0	3
Lime and labour to rubbing up and finishing the outside face of the wall	0	0	3
Total finished and faced on one side	0	2	8
If a wall to a garden or otherways, and finished and faced on both sides, then add	0	0	8
Total for walls finished on both sides	0	3	4

At this place the value of a yard of brick-work is more than ten shillings, of walling only 14 inches thick, the bricks being 42*s.* per 1000, and lime 8*d.* per bushel; consequently the œconomy of the pisè must appear; and the same difference will be found in any other place where lime and bricks bear the same price, and proper earth can be found at hand. But as attempting this sort of work, where it is not applicable, or improperly doing it, so as to lead to failure, may prevent its introduction where it would be useful, I shall endeavour to point out any precautions that have struck me, and every thing that has appeared to make against it.

All earth will not do.

Many persons have supposed, and it has been asserted, that almost any earth will do, but such is certainly very erroneous; for proper earth cannot in all places be found, and it being difficult to describe it, or to be sure when it is found, it seems advisable, before the entering on any considerable work, that the experimentalist should first do a small

piece,

Previous trial recommended. piece, and let it stand with the top only covered for a winter at least.

Requires care It has before been observed, that the excellence of the work depends on its having due compression, as well as being of proper soil. If the compression be not perfect, although the soil be good, the walls will be unsound; and unfortunately it so happens, that when a wall is built and badly rammed, its imperfection cannot readily be observed; and farther, the defect is likely only to be found but by its failure. Hence arises the greatest bar to its general introduction; for, as it requires considerable labour to build a wall, it requires exertion to do it in proper season; and if the labourer be employed to do the work by task, it becomes his interest to get on and do it slightly, and if done by day, it will not advance so rapidly; consequently, in either way, it will require great attention from a careful overlooker. and attentive overlooking.

The saving in expense solely in the walls. From the foregoing comparative statement of pisè against brickwork, persons unacquainted with building are inclined to suppose, that the whole expense of the building will be in proportion thereto; contrary to this it only effects the walling,—the roof, floor, &c., remaining the same as before, excepting as it may reduce the quantity of bond timber and lime used in plastering the inside; this latter is less than when plastered on brickwork, the face of the wall being so much truer than brickwork.

A working drawing, on a scale of one inch to a foot, is left with the Society, for the inspection of any person inclined to construct the apparatus.

XIII.

Curious Property of the Toad. Communicated by a Correspondent.

To Mr. NICHOLSON.

SIR,

AMONG the many curious accounts that I have read of the Toad, I never met with the following fact.

A person

The toad capable of sustaining very great pressure.

A person in the neighbourhood of Maidstone, who manufactures brown paper, informed me, last summer, while I was observing his people at work, that he had frequently placed a toad in the midst of the pile of sheets to be pressed, and always found it alive and well on taking it out, although it must have sustained, with the paper, a pressure equivalent to several tons; but a frog could never survive the same degree of pressure. I searched a long time for a toad, to see the experiment myself; but it was a very warm day, and I was unable to find one, till after the men had left work.

I am, Sir,
Your obliged,

March 25th, 1810. A. M.

XIV.

Communications on the Mode of Action of the Galvanic Pile announced. By J. A. De Luc, *Esq. F. R. S. &c.*

To Mr. NICHOLSON.

SIR,

Discoveries concerning the action of the galvanic pile announced.

YOUR kindness in admitting into your celebrated Journal some former papers of mine induces me to offer you some others; as my view to bring to the knowledge of natural philosophers my discoveries concerning the *Mode of Action* of the *Galvanic Pile* will be as effectually accomplished by that channel, as through the Phil. Trans. of the Royal Society, into which (I know not why) they have not been admitted. The following is a short account of what has passed on this occasion.

Two works on natural philosophy presented to the Royal Society.

In January 1806, I had the honour of presenting to the Royal Society two works, connected with each other, under the titles of, *Introduction à la Phys. terrestre par les Fluides expansibles*, and *Traité élémentaire sur le Fluide electro-galvanique*, which works I wrote at Berlin during my stay in that place, and which were printed at Paris; the latter of them contains a Process of Analysis of the Galvanic Pile, which, on account of my design to make geological observations in various parts of Germany, I had left unfinished, in hope that this process would be followed by some experimental philosopher; which however has not been the case.

and a paper on galvanism.

On my return to Windsor, where I had resumed this rich analysis, I saw, in Part I of the Phil. Trans. for 1807, a Bakerian Lecture of Mr. Davy, *On some Chemical Agencies of Electricity*, read the 20th of Nov. 1806; in which, intermixed with very ingenious experiments and important discoveries in practical chemistry, I found some principles laid down as foundations of a theory, which had already been opposed by the experiments related in the last of the abovementioned works, deposited in the Royal Society's library the beginning of the same year. This engaged me to follow more closely the analysis which I had resumed, and, having completed it, I wrote a paper on this subject, which was presented to the Royal Society the 30th of May, 1808.

This abridged.

Having heard that this paper had not been read, and being told, that it was on account of its length, I took an opportunity of seeing Sir Jos. Banks at Windsor in the beginning of 1809, to beg of him, that this paper should be sent back to me, in order that I might contract it, which he did very obligingly. One of the abbreviations that I adopted was to suppress the application of my experiments to Mr. Davy's theory; and by this and other omissions, having reduced my paper to twenty-three pages, I sent it again to the Royal Society the 25th of Feb. 1809.

Chemical effects of the pile separable from the electrical. New meteorological instrument.

One of the important results of the experiments contained in this paper is, that, in the *Galvanic Pile*, the *chemical* effects can be separated from the *electrical*; and that the latter lead to a new *meteorological* instrument, very desirable for the knowledge of atmospheric phenomena. This object I followed subsequently to the first presentation of my paper, and on the 7th of March 1809, I presented to the Royal Society another paper under this title: *On the Electric Column, and Aerial Electroscope.*

Publication of the papers deferred by the Royal Society.

The following summer, I received a letter, dated from *the Apartments of the Royal Society* the 6th of July, signed Humphry Davy, S. R. S. Saying: *that the Committee of Papers, although they did not think it proper to publish my papers*

papers at present, had directed that they be deposited in the Archives of the Society. And at the same time I was informed, that this had been decided from the minutes only of the Secretary Dr. Wollaston, which certainly could not give a sufficient knowledge of papers consisting of descriptions of experiments and of immediate results. Therefore, at the next meeting of the Royal Society, I sent to the President a petition to the Committee, dated the 5th of Nov. 1809, in which, founded on the above circumstance, I claimed a revisal of their decision, by judging of the papers themselves. But soon after this petition was sent, I was assured, that, whatever might be my reasons for the claim of a revisal, I should not obtain it, the Committee never changing their first resolution; and this proved to be the case, as I was informed on the 25th of December. I wrote the 28th, begging that the drawings annexed to these papers should be lent me, in order to have them engraven, as I meant to publish my papers myself; but to this day I remain without an answer.

Electric column.

In the mean time I had made no secret of my discoveries. I had communicated the *electric column* and its construction to many experienced philosophers, and in particular, being in London in July 1808, I showed it to Mr. Davy. From this, and the reading the minutes both in the Royal Society and in the Committee, the construction and phenomena of the *electric column* are much known, and it has been executed by many experienced philosophers. Thus, a publication of this instrument by me will at present be deprived of the merit of novelty; but only with respect to its immediate and surprising phenomena: for the chief importance of the *electric column* is its connection with the *galvanic pile*, hitherto consigned only to my detained papers and to the memory of those of my friends, to whom I have explained it.

The subject promised in a more minute state.

In proposing to you, Sir, to be the channel through which natural philosophers at large may be informed of these discoveries, I do not intend to offer you the publication of these papers, as they were sent to the Royal Society; for they had been composed during the progress of my experiments, which produced a form at present not so convenient. The progress of discoveries is always interesting in the history of science; and with respect to this object, my papers, deposited in the archives of the Society, may certify the date of the steps; which however were not completed, for since that time I have carried them further. Therefore, for your Journal, if you accept my offer, I shall take the subject as it now stands, dividing it under three heads: *Analysis of the Galvanic Pile.—Production of an Instrument giving spontaneous signs of Electricity, without chemical Effects.—Analysis of the Electric Machine, compared with that Instrument.*

If you are disposed, Sir, to undertake this publication, I shall not have recourse to any other method of giving these experiments to the public. You may insert this letter in your next number, in order to announce the subject; for the following number, you shall have the first of the above papers, and the others in monthly succession. To each paper a drawing will be annexed, but I shall make these as simple as possible.

I shall be very happy to have with you this new connexion, and I remain, Sir,

Your obedient humble servant,

Windsor, DE LUC.
22d March, 1810.

Annotation.—This letter is inserted at the request of the learned author, to whom I have answered, that I shall be happy to pay every attention to his communications, which my duty as a Journalist, and the nature and importance of those writings, may demand. W. N.

XV.

Observations on Galvanic Batteries. In a Letter from Mr. Charles Sylvester.

To Mr. NICHOLSON.

Dear Sir, *Derby, April 17th,* 1810.

Facts in the article Galvanism in Nich. Enc.

THE article Galvanism, which I had the honour to write for the Encyclopœdia lately published by you, contains some facts which had not before appeared in print, and, consequently, were not backed by any specified authority. As the authenticity of them might on this account be questioned, I beg to state, through the medium of your Journal, that they were developed during a series of experiments, in which I was engaged several years ago, and have been very frequently exhibited in my lectures upon the subject. In my present communication, I shall take the opportunity of recording one or two others, which did not occur to me at the time I wrote that article, and which, in a practical point of view, may not be deemed altogether unimportant.

observed by Mr. Sylvester.

About three years since, I devoted some time to the investigation of the subject of galvanic batteries, and have now in my possession one made at that period; the cells of which are formed with glass plates, so as to allow the plates of metal to be taken out and put in at pleasure. A similar plan is at present in use, and these batteries are said to possess the advantage of exposing double the surface of the common ones. I did not find this the case by any means, in my inquiry into the subject, and it was under this idea, that I did not persevere in the discovery. The reason I give for these batteries not having double power is, that two of the surfaces of each pair of plates are not contiguous. I have since that time obviated this evil, and hope very soon to give you an account of a battery, considerably more improved in construction. A fact, which furnishes a useful lesson upon this point, is the following: let two wine glasses be nearly filled with dilute muriatic acid; and afterward immerse in them an arc composed of a piece of zinc and a piece of copper wire; the zinc being in one vessel, and the copper in the other. If another arc, formed of similar wires, be then plunged into the glasses, in a reversed order, observing that the arcs do not touch each other, a very rapid decomposition will take place, and hidrogen will be evolved from each of the copper wires. Let one of the above arcs be now removed, and the connexion be made by means of an arc consisting of one metal only; the copper wire of the *compound* arc will be seen still to afford bubbles of hidrogen, but in much less quantity. Instead of this *simple* arc, employ

Battery with the metals separated by glass plates

has not double the power of the common ones.

Fact apparently proving, that the copper surface should be greater than the zinc.

ploy next a slip of copper of a conical shape; and on reversing it, a very striking difference will be observed. When the small end of it is placed in the glass containing the zinc wire, no greater effect will be apparent, if it present nearly the same surface, than when the copper wire was used as the connecter; but, when the broad end occupies the same situation, the effect in decomposing water will be found increased to a very considerable degree. This experiment appears to prove, that the copper surface, in galvanic batteries, ought to be greater than the zinc surface, in order to produce the greatest possible effect.

Effects of a galvanic battery counteracted by a coating of mercury in the metal,

which arose from the muriatic acid being contaminated by corrosive sublimate.

This impurity more common than suspected.

I shall conclude my present letter by mentioning a curious anomaly with respect to the agency of mercury in galvanic combinations, which I do not recollect to have before seen noticed. For the purpose of showing the decomposition of water by galvanism, I have been accustomed to employ wires of various metals connected with zinc, using dilute muriatic acid as the oxidating medium; and have found silver, copper, bismuth, cobalt, arsenic, as well as many others, answer the purpose extremely well. In making the experiment at one of my lectures some time ago, however, I was surprised to find, that the usual effects were not produced, and was very long at a loss to conceive, what could be the cause of so curious a circumstance. By an attentive examination of the phenomena, I perceived, that the wire which ought to have furnished the hidrogen had become coated with a white matter, which, on subsequent research, proved to be mercury, and had been obtained from the reduction of corrosive sublimate, which entered, as an impurity, into the composition of the muriatic acid I had employed. Since that period, the accident has happened to me twice, in distant places, from the same cause; a fact, which seems to point out corrosive sublimate as a more frequent ingredient in marine acid, than might be at first suspected. This experiment contributes to the support of an opinion, that galvanic effects are not entirely dependent upon the relative oxidability of the metals employed, as has been hitherto generally supposed; for although, in the above case, the quicksilver was nearly as oxidable as the metal which it coated, yet the activity of the galvanic influence was interrupted.

I am, Sir,
Your obedient servant,
CHARLES SYLVESTER.

XVI.

Remarks on Professor Leslie's Doctrine of radiant Heat; with Experiments to show, that Caloric can pass through transparent Media without heating them. By J. D. MAYCOCK, *Esq.*

To Mr. NICHOLSON.

SIR,

ALTHOUGH the most approved systematical writers on chemistry have not adopted the opinions of Mr. Leslie, respecting the manner in which an equilibrium of temperature is preserved among distant bodies, they have conceived the result of his experiments sufficient to prove, that dense media are impervious to caloric radiated from surfaces artificially heated.

Dense media supposed impervious to radiating heat,

I shall not enter into a minute examination of the merits of the hypothesis, proposed in the "Experimental Inquiry," for explaining what is commonly termed the radiation of caloric; but shall content myself with reminding you, that it rests entirely on a supposition, that caloric passes through no medium without first raising its temperature: the truth of which Mr. Leslie considers as proved by the effects of different skreens interposed, in his experiments, between the thermometer and the source of heat. But as in the experiments alluded to no regard was paid to time, I hold all reasonings founded on them as fallacious and unsatisfactory; and having myself obtained results, which favour opinions very opposite to those advanced by the learned professor, I take the liberty of communicating them to you.

and caloric to raise the temperature of every medium it passes through.

Exp. 1. I placed two metallic reflectors, 12 inches diameter, and 5½ in. focal distance, fronting each other, and 30 in. distant; I brought a differential thermometer into the

Experiments to prove the contrary.

the focus of one, a lighted candle into the focus of the other reflector. In two minutes the thermometer fluid rose 63°. This I determined by taking the mean of six experiments, the results of which, from attention to the burning of the candle, were very nearly alike.

I procured three skreens; one of painted glass, one of painted tin (both black), the other of plain glass. They served for the following experiments.

Exp. 2. *Exp.* 2. I placed a skreen stand midway between the two reflectors. On this I fixed the painted glass skreen; in two minutes the thermometer rose 2°: repeated, 2°: repeated, 1⅔°.

Exp. 3. *Exp.* 3. I exchanged the painted glass for the painted tin. In two minutes the thermometer rose 1⅔°: repeated, 1⅔°: repeated, 1⅔°.

Exp. 4. *Exp.* 4. Having removed the painted tin, and interposed the plain glass, the thermometer rose in two minutes 25°: repeated, 26°: repeated, 25⅔°.

Exp. 5. *Exp.* 5. The apparatus remaining as in the preceding experiments, and the glass skreen resting on the stand, when the thermometer had fallen to zero, I brought the lighted candle into the focus of the first reflector; the thermometric fluid mounted in two minutes to 24°. I removed the candle, but left the skreen. The thermometer did not rise in the slightest degree, and in 30″ it had fallen 3°; in 45″, 8°; in 60″, 12°; in 75″, 14°; in 90″, 16°; in 105″, 18°; and in 2′, 19°.

Exp. 6. *Exp.* 6. I substituted the painted for the plain glass. In two minutes the thermometer was at 1⅓°. I removed the candle, the skreen remaining on the stand; in one minute the thermometer rose 1½° more, and after a short time it began to fall.

Exp. 7. *Exp.* 7. Having fixed the blackened glass skreen on the stand, I brought into the focus of the first reflector a 7 oz. flask, containing 4½ oz. of a boiling mixture of sulphuric acid and water, the boiling point of which was somewhere between 450° and 500° Fah. In two minutes the thermometer rose 1¾°. I removed the flask—the thermometer continued to rise until at 1¼° of the scale.

Exp. 8. *Exp.* 8. I interposed the plain instead of the painted glass. In two minutes the thermometer was up to $2\frac{1}{4}^\circ$. I removed the hot flask; the thermometer did not experience any farther rise, and in two minutes it had fallen $\frac{1}{2}^\circ$.

Exp. 9.

Exp. 9. A 7 oz. flask of boiling water being used as the source of heat, I found, that plain glass transmitted caloric as 7 to painted glass as 5; and that, after the hot flask was removed, the effect of the black glass skreen was to that of the plain glass as 15 to 13.

Exp. 10.

Exp. 10. Having painted one side of a pane of glass black, I placed it on the skreen stand with its blackened surface next to the candle. In two minutes the thermometer rose 2·3°.

Exp. 11.

Exp. 11. I opposed the clean surface of the same skreen to the hot body. In two minutes the thermometer stood at 3·8°.

Exp. 12.

Exp. 12. I removed one of the reflectors, and found, that, without the skreen was very near to the source of heat, no sensible effect was produced on the thermometer in two minutes. The skreen stand being situate one inch from the hot body, glass did not appear to transmit caloric better than any other medium.

Objections answered.

Before I attempt to draw any conclusions from the preceding experiments, I think it necessary to anticipate some objections, which may possibly be urged to the manner, in which the most striking were made.

Inequality of the heat of a candle.

The inequality of heat produced by the combustion of a candle may be thought a very fertile source of errour: and I am ready to allow, that the flame of a candle could not with propriety be employed to determine minute differences in the temperature of the thermometer. But the differences I observed were very far from being minute, and the experiments, as often as repeated, afforded similar results.

That light is supposed to be heating,

The burning candle will be deemed particularly objectionable by all those who contend, that light itself is possessed of a heating property. It would be entirely foreign to my present purpose, to attempt a refutation of this purely hypothetical opinion. I would however avail myself of the excellent experiments of Dr. Herschel, which show, that the heat excited by the different prismatic rays is in no degree proportioned to their illuminating power, the greatest effect on the thermometer being produced half an inch beyond the visible spectrum. Indeed Dr. Herschel's several papers, published in the Ph. Trans. for 1800, render it, at least, extremely probable, that the colorific rays themselves do not affect the temperature of bodies. Now it is matter of little consequence, whether we term the invisible heat-making rays light or caloric. If we are pleased to call them light, we must yet admit, that they are invisible; and cannot therefore, because the candle's flame is luminous, object to its being used as a source of heat in experiments on radiant caloric.

as may be thought to render bodies more pervious to heat.

It may still be urged, that, although light, strictly so called, is not possessed of a heating property, yet its association with caloric may cause this agent to pass through media, which under other circumstances would be impervious to it. It is only possible to conceive the transmission of caloric through dense media to be facilitated by its association with light, by supposing an attraction to be exerted by these fluids for each other. But all the phenomena of phosphorescence show, that the very opposite to attraction, namely repulsion, prevails between light and caloric. All objections, however, to the source of heat will be removed, when we consider, that the experiments in which the flask with a boiling mixture of sulphuric acid and water was used afforded results similar to those obtained from the combustion of a candle.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineralogy of Strontian and Ben Nevis.

AT the meeting of this Society on the 10th of March, the Rev. Dr. Macknight read a paper on the mineralogy of Strontian and Ben Nevis. The rocks which compose the districts of Strontian are mica slate, gneiss, and granite; and the lead-glance, which occurs in gneiss, is associated with iron-pyrites, cross-stone, calc-spar, foliated zeolite, strontian, and heavy-spar. Ben Nevis is an overlying massive formation, which rests on gneiss and mica-slate, approaching in some places to clay-slate. In this formation, compact feldspar is the leading ingredient. The inferior mass consists of sienite, passing from the simple-granular to the granular-porphyritic: and the upper portion of the mountain, comprehending the summit, with about 1400 feet of the perpendicular hieght below it, is composed of a dark-coloured rock, which for the most part is porphyritic, and seems to be intimately allied in its characters to compact feldspar. This appears from the gradual transition of the one substance into the other, which is distinctly observed under the tremendous precipice of Ben Nevis to the N. E., and demonstrates the identity and continuity of the whole formation. The colouring matter appears to be hornblende intimately mixed with the substance of the rock. At first view the whole might have been considered as a formation of clink stone and porphyry-slate. But a more minute investigation discovers many oryctognostic characters of distinction from these substances, which are less crystalline, and belong to a more recent era of formation.

Zetland sheep.

At the same meeting, Dr. Arthur Edmonstone read an account of the peculiarities of the Zetland sheep, with remarks on their diseases. And the Secretary read a communication from Lieut. Col. Imrie, describing a vein of greenstone, which occurs in Glenco, and which appears to have been overlooked in the mineralogical descriptions of that district.

Vein of greenstone in Glenco.

Literary and philosophical society at Hackney.

The establishment of societies for the promotion and diffusion of knowledge by reading and conversation, wherever circumstances admit them, cannot fail to promote the general interests of science, and I am happy therefore to announce the formation of a Literary and Philosophical Society at Hackney. The object of this society is not confined to the purchase of books, the use of which the members are allowed at their own houses; but its plan extends to procuring philosophical instruments, and patronising lectures on philosophical subjects, as soon as the state of its funds will permit. A weekly meeting is likewise to be held for literary conversations, and reading such papers as the society may be favoured with. The subjects for conversation, or books for the library, comprehend the mathematics, natural philosophy, natural history, chemistry, polite literature, antiquities, history, biography, questions of general law and policy, commerce and the arts; but topics of religion, the practical branches of law and physic, and the politics of the day, are excluded. Its commencement has been very auspicious.

New edition of Dr. Henry's chemistry.

A new edition of Dr. Henry's Chemistry is in the press. As the late grand discoveries of Mr. Davy have wrought extraordinary changes in the face of the science, the present may be considered almost as a new work, from the various alterations that have been necessary, and the great additions required to detail and explain the progress that has been made. It will now extend to two volumes, which are printing with all possible expedition.

METEOROLOGICAL JOURNAL,

For APRIL, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAR. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Day.	Night.
28	45°	45°	50°	37·5	29·70	Rain	Rain
29	44·5	45	52	37	29·93	Ditto	Fair
30	43·5	44·5	48	41·5	29·94	Ditto	Cloudy
31	46·5	46	51	41·5	29·76	Ditto	Rain
APR.							
1	45·5	45	49	41	29·45	Rain	Rain
2	44	44·5	48.5	43·5	29·76	Cloudy	Cloudy *
3	49	50	54·5	40	29·64	Rain	Rain †
4	45	43	47·5	35·5	29·44	Ditto	Fair
5	42	45	50	41·5	29·76	Fair	Cloudy
6	44	43·5	49	41·5	29·37	Rain	Ditto
7	46	47	53·5	41·5	29·32	Showery	Fair
8	46·5	47	52	43·5	29·49	Fair	Rain
9	47	46	51	42	29·48	Rain	Ditto
10	44	43	45	35	29·53	Ditto	Ditto
11	40	38	42	32	29·67	Showery	Fair ‡
12	34·5	37·5	42·5	31	29·92	Fair	Ditto
13	36	37	41·5	34	29·94	Ditto	Cloudy §
14	39	41	45·5	38	29·90	Cloudy	Fair
15	45	43·5	52	38	29·83	Fair	Ditto
16	44·5	42·5	52·5	37·5	29·56	Rain	Cloudy
17	45	46·5	53	42·5	29·49	Showery	Fair
18	49	54	60	45·5	29·65	Fair	Ditto
19	51	52·5	59·5	45	29·76	Ditto	Ditto
20	51·5	52	57·5	44·5	29·97	Ditto	Ditto
21	51·5	54·5	61	43	30·20	Ditto	Ditto
22	51	55	63	47	30·23	Ditto	Ditto
23	54	55	64·5	46·5	30·21	Ditto	Ditto
24	52·5	53	62·5	45	30·24	Ditto	Ditto
25	52	48	62	42·5	30·19	Ditto	Cloudy ‖
26	51·5	50	59	43	30·14	Ditto	Fair

* Rain at 11, and all the night. † At 11 stars visible.

‡ The day cold with hail and rain.

§ The day and evening cold. Small fall of snow at 9 P. M.

‖ Evening cold.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1810.

ARTICLE I.

On the Volcanoes of Jorullo; by ALEXANDER HUMBOLDT*.

Large hill thrown up by a volcano in 1759.

THE grand catastrophe in which this volcanic mountain issued from the earth, and by which the face of a considerable extent of ground was totally altered, was perhaps one of the most extensive physical changes, that the history of our globe exhibits. Geology points out spots in the ocean, where, within the last two thousand years, volcanic islets have arisen above the surface of the sea, as near the Azores, in the Archipelago, and on the south of Iceland: but it records no instance of a mountain of scoriæ and ashes, 517 met. [563 yards] above the old level of the neighbouring plains, suddenly formed in the centre of a thousand small burning cones, thirty-six leagues from the seashore, and forty-two leagues from any other volcano. This phenomenon remained unknown to the mineralogists and natural philosophers of Europe, though it took place but fifty years ago, and within six days journey of the capital of Mexico.

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* Extracted from his Essay on New Spain. Journal de Physique, vol. LXIX, p. 149.

Country described.

Descending from the central flat toward the coasts of the Pacific ocean, a vast plain extends from the hills of Aguasarco to the villages of Toipa, and Patatlan, equally celelebrated for their fine cotton plantations. Between the picachos del Mortero and the cerras de las Cuevas and de Cuiche, this plain is only from 750 to 800 met. [820 to 880 yards] above the level of the sea. Basaltic hills rise in the midst of a country, in which porphyry with base of greenstone predominates. Their summits are crowned with oaks always in verdure, and the foliage of laurels and olives intermingled with dwarf fan palms. This beautiful vegetation forms a singular contrast with the arid plain, which has been laid waste by volcanic fire.

A fertile plain

To the middle of the eighteenth century fields of sugarcanes and indigo extended between two rivulets, called Cuitimba and San Pedro. They were skirted by basaltic mountains, the structure of which seems to indicate, that all the country, in remote periods, has several times experienced the violent action of volcanoes. These fields, irrigated by art, belonged to the estate of San Pedro de Jorullo (Xorullo, or Juvriso), one of the largest and most valuable in the country. In the month of June, 1759, fearful rumbling noises were accompanied with frequent shocks of an earthquake, which succeeded each other at intervals for fifty or sixty days, and threw the inhabitants of the estate into the greatest consternation. From the beginning of the month of September, every thing seemed perfectly quiet, when in the night of the 28th of that month a terrible subterranean noise was heard anew. The frightened Indians fled to the mountains of Aguasarco. A space of three or four square miles, known by the name of Malpays, rose in the shape of a bladder. The boundaries of this rising are still distinguishable in the ruptured strata. The Malpays towards the edge is only 12 met. [13 yards] above the former level of the plain, called las playas de Jorullo; but the convexity of the ground increases progressively toward the centre, till it reaches the height of 160 met. [175 yards].

shaken by an earthquake,

and a hill raised on it.

They

The event described.

They who witnessed this grand catastrophe from the top of Aguasarco assert, that they saw flames issue out of the ground for the space of more than half a league square; that fragments of red hot rocks were thrown to a prodigious height; and that through a thick cloud of ashes, illumined by the volcanic fire, and resembling a stormy sea, the softened crust of the earth was seen to swell up. The rivers of Cuitimba and San Pedro then precipitated themselves into the burning crevices. The decomposition of the water contributed to reanimate the flames, which were perceptible at the city of Pascuoro, though standing on a very wide plain 1400 met. [1530 yards] above the level of the playas de Jorullo. Eruptions of mud, particularly of the strata of clay including decomposed nodules of basaltes with concentric layers, seem to prove, that subterranean waters had no small part in this extraordinary revolution. Thousands of small cones, only two or three yards high, which the Indians call ovens, issued from the raised dome of the Malpays. Though the heat of these volcanic ovens has diminished greatly within these fifteen years, according to the testimony of the Indians, I found the thermometer rise to 95° [if centig. 203° F.] in the crevices that emitted an aqueous vapour. Each little cone is a chimney, from which a thick smoke rises to the height of ten or fifteen met. [11 or 16 yards]. In several a subterranean noise is heard like that of some fluid boiling at no great depth.

Six large hills in one line.

Amid these ovens, in a fissure, the direction of which is from N. N. E. to S. S. E., six large hummocks rise 400 or 500 met. [440 or 550 yards] above the old level of the plain. This is the phenomenon of Monte Novo at Naples repeated several times in a row of volcanic hills. The loftiest of these huge hummocks, which reminded me of the country of Auvergne, is the large volcano of Jorullo. It is constantly burning, and has thrown out on the north side an immense quantity of scorified and basaltic lava, including fragments of primitive rocks. These grand eruptions of the central volcano continued till February, 1760. In the succeeding years they became gradually less frequent. The Indians, alarmed by the horrible noise of the new volcano, at first deserted the villages for seven or eight leagues

Ashes carried to a great distance.

The air still heated by the subterranean fire.

round the plain of Jorullo. In a few months they became familiar with the alarming sight, returned to their huts, and went down to the mountains of Aguasarco and Santa Ines, to admire the sheaves of fire thrown out by an infinite number of large and small volcanic openings. The ashes then covered the houses of Queretoro, more than 48 leagues [120 miles] in a right line from the place of the explosion. Though the subterranean fire appears to be in no great activity* at present, and the Malpays and the great volcano begin to be covered with vegetables, we found the air so heated by the little ovens, that in the shade, and at a considerable distance from the ground, the thermometer rose to 43° [109·4° F.]. This fact evinces, that there is no exaggeration in the report of some of the old Indians, who say, that the plains of Jorullo were uninhabitable for several years, and even to a considerable distance from the ground raised up, on account of the excessive heat.

Two rivers lost,

and supposed to appear again much heated.

Sulphurous water.

Near the cerro of Santa Ines the traveller is still shown the rivers of Cuitimba and San Pedro, the limpid waters of which formerly refreshed the sugarcanes on the estate of Don Andrew Pimantel. These springs were lost in the night of the 29th of September, 1759: but 2000 met. [near 2200 yards] to the westward, in the soil that has been elevated, two rivulets are seen to break out of the clayey dome of the furnaces, exhibiting themselves as thermal waters, in which the thermometer rises to 52·7° [126·86° F.]. The Indians still give these the names of San Pedro and Cuitimba, because in several parts of the Malpays large bodies of water are supposed to be heard running from east to west, from the mountains of Santa Ines to the estate of the Presentation. Near this estate is a brook, that emits sulphuretted hidrogen gas: it is more than 7 met. [near 8 yards] wide, and is the most copious hidrosulphurous spring I ever saw.

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* In the bottom of the crater we found the heat of the air 47° [116·6° F.], and in some places 58° and 60° [136·4° and 140°]. We had to pass over cracks exhaling sulphurous vapours, in which the thermometer rose to 85° [185°]. From these cracks, and the heaps of scoriæ that cover considerable hollows, the descent into the crater is not without danger.

In

The catastrophe supposed owing to the vengeance of some monks.

In the opinion of the natives these extraordinary changes I have described, the crust of earth raised and cracked by volcanic fire, the mountains of scoriæ and ashes heaped up, are the works of monks; the greatest, no doubt, they ever produced in either hemisphere. Our Indian host, at the hut we inhabited in the plain of Jorullo, told us, that some missionary capuchins preached at the estate of San Pedro, and, not meeting a favourable reception, uttered the most horrible and complicated imprecations against this plain, then so beautiful and fertile. They prophesied, that the estate should first be swallowed up by flames issuing out of the bowels of the Earth; and that the air should afterward be cooled to such a degree, that the neighbouring mountains should remain for ever covered with ice and snow. The first of these maledictions having been so fatally verified, the common people foresee in the gradual cooling of the volcano the presage of a perpetual winter. I have thought it right to mention this vulgar tradition, worthy a place in the epic poem of the jesuit Landivar, because it exhibits a striking feature of the manners and prejudices of these remote countries. It shows the active industry of a class of men, who, too frequently abusing the credulity of the people, and pretending to possess the power of suspending the immutable laws of nature, know how to avail themselves of every event for establishing their empire by the fear of physical evil.

Line of volcanoes in Mexico crossing the chain of hills.

The situation of the new volcano of Jorullo leads to a very curious geological observation. It has already been observed in the 3d chapter, that there is in New Spain a line of great heights, or a narrow zone included between the latitudes of 18° 59′ and 19° 12′, in which are all the summits of Anahuac that rise above the region of perpetual snow. These summits are either volcanoes still actually burning; or mountains, the form of which, as well as the nature of their rocks, renders it extremely probable, that they formerly contained subterranean fire. Setting out from the coast of the Gulf of Mexico, and proceeding westward, we find the peak of Oribaza, the two volcanoes of la Puebla, the Nevado de Toluca, the peak of Tancitaro, and the volcano of Colima. These great heights, instead of forming the ridge of the cordillera of Anahuac, and following its direction, which is from S. E. to N. W., are on the contrary in a line perpendicular to the axis of the great chain of mountains. It is certainly worthy remark, that in the year 1759 the new volcano of Jorullo was formed in the continuation of this line, and on the same parallel as the ancient Mexican volcanoes.

Indicate a long interior fissure in the Earth.

A view of my plan of the environs of Jorullo will show, that the six large hummocks have risen out of the earth on a vein, that crosses the plain from the cerro of las Cuevas to the pichaco del Montero. The new mouths of Vesuvius too are found ranged along a fissure. Do not these analogies give us reason to suppose, that there exists in this part of Mexico, at a great depth within the Earth, a fissure stretching from east to west through a space of 137 leagues [343 miles], and through which the volcanic fire has made its way at different times, bursting the outer crust of porphyritic rocks, from the coasts of the Gulf of Mexico to the South Sea? Is this fissure prolonged to that little groupe of islands, called by Colluet the Archipelago of Regigedo, and round which, in the same parallel with the Mexican volcanoes, pumice stone has been seen floating? Naturalists who distinguish the facts offered by descriptive mineralogy from theoretical reveries concerning the primitive state of our planet will pardon me for having consigned these observations to the general Map of New Spain, contained in the Mexican Atlas.

II.

Chemical Inquiry into the Composition of some Weapons and Utensils of ancient Bronze: by Mr. KLAPROTH *.

Copper prefered to iron by the ancients.

WE know from ancient authors, as well as from weapons and utensils dug up in modern times, that men in the earliest ages, and even those that succeeded them, employed copper in preference for the fabrication of metallic utensils and weapons. Thus what Herodotus says of the Massagetæ, who used no iron, and whose weapons and utensils were of copper, is more or less applicable to all the nations of antiquity.

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* Magazin Encycloped. Juin, 1808, p. 298. From Gehlen's Chemical Journal.

They were sooner acquainted with it, as more easily wrought.

The great difference in the exterior characters of the two metals in their crude state leaves no doubt, that men were sooner acquainted with copper, and the method of adapting it to their purposes, than iron. It is probable, that they found copper in large masses, and nearly prepared by nature, as we still meet with it in countries, the mineralogical wealth of which has been little explored. Accordingly in treating the ore by fire they could not fail to observe all the advantages of this metal, both with respect to the richness of its produce, and the facility with which it might be forged. Iron, on the contrary, was not so obvious to men's eyes; and the distinguishing of its various ores, with the art of working them, and forming weapons and instruments of them, could only be the fruit of long experience.

Copper employed in preference to iron, when the latter was well known.

I shall not avail myself of the numerous testimonies of ancient authors to prove, that copper has been employed in preference to iron, as it is sufficient, to appeal to Homer. All weapons, both offensive and defensive, as swords, spearheads, helmets, and shields, as well as various domestic utensils, were of copper (καλχος); though in Homer's time iron (σιδηρος) was used, but less frequently, and hardened by plunging redhot into water. Even when the advantages of iron, and the modes of fabricating it, were well known, men used copper for their weapons; for instance, in the last ages of the republics of Greece and Rome.

The copper was hardened by a mixture of tin.

We know, that copper is not fit for the purposes, for which the ancients employed it. When cast it is porous and brittle; and, when forged, too soft. The ancient weapons and utensils being of a hardness, which this metal does not possess, it was long supposed, that the ancients had some method of hardening copper, as we do iron and steel. But chemical analysis has shown the falsity of this opinion; and demonstrated, that these weapons and instruments were not pure copper, but an alloy of this metal with tin, which we call bronze, and which was the *æs*, brass, of the Romans. The

This was done in the oldest times

The weapons, instruments, and statues, which have been dug out of the ground, evidently prove, that the property of tin to impart hardness and density to the metal alloyed with it was known and employed by the most ancient nations. All these objects occur of bronze, but none of pure copper. It is astonishing, that this practice of imparting to copper, by alloying it with a certain portion of tin, a hardness sufficient for sword-blades and other cutting instruments, should have been so generally followed by the ancients, notwithstanding the want of tin mines. All the tin they used they were obliged to procure from the Cassiterides, the present Cornwall, and the trade was exclusively in the hands of the Phenicians.

The tin fetched from Cornwall.

Various antiquities analysed.

Having had an opportunity of assaying several fragments of metallic antiquities, I conceive it may be of some utility to make public the results, as a supplement to the few accurate analyses hitherto made.

Mode of analysis.

The fragments to be analysed, being first weighed, were put into a phial, into which were poured six or eight parts of nitric acid of the specific gravity of 1·22, and digested in a sandheat, till completely dissolved. The contents of the phial were then diluted with a sufficient quantity of water, and the mixture left to stand, till all the oxide of tin had fallen down, and the azure liquid appeared quite clear. This being poured off, the oxide of tin was collected, washed repeatedly with water, dried, heated redhot, and weighed. It was found, that 100 parts of calcined oxide of tin equalled 80 parts of tin in the metallic state. The nitric solution was tested in the usual way for silver, iron, lead, and zinc. When it was found free from these metals, as in all the following inquiries it proved, it was easy to calculate, by deducting the quantity of tin found, the proportion of copper, which was likewise obtained by the common methods.

1. *Analysis of an antique sword.*

An antique sword described.

In a collection of antiquities at Berlin, found on digging into some ancient graves in the march of Brandenburg, among several articles of bronze, as spear-heads, knives, ornaments, &c., are two swords; but the place where they were found is not known. One of these swords was broken, the other entire. Their composition is the same. They are both covered with the green shining rust called patina. The sword in question weighs 17 ounces, and is 20 inches long; the blade 16½, and the hilt, which is rivetted, 3½*. The blade is twoedged, and 1¼ inch broad for two thirds of its length, the other third sloping off to a round point. In the middle it is 3½ lines thick, and slopes to form an edge on each side. Below the hilt, and on each side at the edge, is a part cut out 9 or 10 lines long, and 1 or 1¼ deep, the use of which I do not know.

To find the colour and brightness of the blade, I ground it; and though the parts injured by the rust prevented me from restoring its original appearance, its colour and lustre were observable in some parts, and indicated considerable hardness and density.

Its composition.

Its analysis in the way above mentioned gave the following proportions: copper 89, tin 11.

Compared with three others found in France.

To render the description and analysis of these antique swords found in our country more interesting by a comparison with other weapons, I shall here give an abstract of two excellent papers by Mr. Mongez in the 5th vol. of the Memoirs of the Institute, which contain a description and analysis of similar bronze swords found near Abbeville. One was found under a bed of peat, with the skeletons of a man and horse. Its whole length was 22 inches, the breadth of the blade 16 lines, the weight 21 ounces. According to the analysis of Mr. Darcet, it contained 15·53 tin, and 87·57 copper †. A second, which was found at the depth of 10 feet, in a calcareous tufa, was about 29 inches long, and contained fifteen tin and 85 copper. The rivets that fastened the outer part of the hilt contained but 5 per cent of tin, because they required to be more flexible. A third was 33 inches long, and found at the depth of nine feet in a bed of peat, by the side of the skeleton of a man, on the head of which was a bronze helmet. This skeleton and several others were lying in an ancient boat. The composition of this sword was 10 tin and 90 copper. Another sword, or cutlas, 18½ inches long, contained but 4 per cent of tin.

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* As I do not know, whether the measures in this paper be French or German, they are left as in the French Journal. C.

† One of these two numbers is evidently wrong, but I know not which. C.

The bronze instruments of the ancients were cast not forged.

These antique swords were not forged, like our weapons of iron and steel, but were cast in moulds, like all other instruments of bronze. Their edge, as well as those of cutting instruments in general, must have been given by hard, smooth stones. The opinion of some antiquaries therefore, who assert, that the ancients were unacquainted with the art of casting metals, is absolutely false.

To say nothing of the nature of bronze rendering it incapable of being prepared in any other way, any one may be convinced of this by simple inspection; and if you would have a proof of it in Homer, you need only read the 23d book of the Iliad.

2. *Analysis of the metallic alloy of crooked antique knives.*

Crooked antique knives.

In several provinces of Germany cutting instruments, shaped like sickles, have been found in digging or ploughing the ground; but whether they really were ancient sickles is not determined, as many suppose, that they may have been used as knives in the warm baths. I have selected two of these, found at different places, for analysis.

One, which was found with various utensils in a garden at Merz, near Mueltord, yielded by analysis, after its crust of grayish rust was removed, tin 15 parts, copper 85.

The other, found in the island of Rugen, was covered with the common patina, and gave tin 13, copper 87.

3. *Analysis of an antique ring.*

An antique ring, of elastic and flexible bronze.

I had selected for other inquiries a fragment of an elastic and flexible ring, which was found with some Roman coins in the vicinity of the Rhine. This ring was made with a half-flattened stem, grooved on the outside, and 8 lines broad. Its exterior diameter is 2¾ inches, its interior 2¼. It is not soldered, but its extremities are so closed by the elasticity of the metal, that it is difficult to separate them. The colour of the metal, in the parts that have been polished, is very fine. We have no sufficient clew to the use of these rings. Its analysis gave tin 9, copper 91.

The same proportions were found in an elastic ring analysed by Mr. Mongez, which was found near Bourg, where several other Roman antiquities had before been discovered.

It is to be wished, that the elastic property of bronze should be examined more minutely.

4. *Analysis of a piece of Grecian brass.*

Grecian brass.

This little fragment, decorated with ornaments, which was found in Sicily in a Grecian tomb, appears to have been a button, or some other ornament of armour. Its proportions are, tin 11, copper 89.

5. *Analysis of antique rivets.*

Antique rivets.

These rivets were short, and of the thickness of a middle-sized wire. As it was necessary they should be flexible, it was requisite, that the alloy should be in different proportions, that of the tin being diminished. This consisted of tin 2·25, copper 97·75.

6. *Analysis of an antique cup.*

Antique cup.

The great number of antique cups and vases found at different times sufficiently prove, that the ancients possessed the art of reducing bronze to thin sheets. The cup, pieces of which were employed for this analysis, was found in a Grecian tomb near Naples. It has so well resisted rust, that its inside has lost very little of its polish. Being very thin, I expected to find in it but a small proportion of tin; but I obtained tin 14, copper 86.

Antique mirror.

Comparing the proportions of tin found in the present analysis with those of a fragment of an antique mirror, which I had already published in Scherer's Journal, vol. VI, and which consisted of 32 per cent tin, and a little lead, we find, that the ancients judiciously adapted the proportions of tin and copper to the purposes, for which they were required. I conceive it unnecessary to particularize the rest of the analyses I made of pieces of antique bronze: it is sufficient to say, that, except this mirror and the rivets already mentioned, I always found the alloy contained from 9 to 15 per cent of tin.

7. *Analysis of the quadriga of Chios.*

Quadriga of Chios.

The proportions of the alloy of this masterpiece of antiquity bear no resemblance to those already mentioned. It has been long asserted, that these horses were the work of Lysippus, contemporary of Alexander, who is known in the history of the arts as the greatest master in the execution of equestrian statues: but several modern connoisseurs dispute this, and say the horses are in too clumsy a style, to have been the work of Lysippus.

History.

It is admitted however, that they were brought from Chios to Constantinople in the reign of Theodosius I. In 1204, when the croisaders made themselves masters of that city for the second time, pillaged it, and set it on fire, this quadriga escaped the destruction, that befel many ancient works of art. On dividing the plunder, the doge Dandolo destined these horses for the republic of Venice. After his death the podestat Martin Zeno sent them to Venice with other parts of the spoil, and the doge Peter Ziani ornamented with them the entrance to the cathedral of St. Mark. About six centuries after, in 1798, they were removed to Paris, and placed at the two entrances of the square of the Carrousel. Since that time they have been brought together again, and harnessed to a chariot, to decorate the triumphal arch in that square.

Fabrication.

These four horses were not cast at once, like statues in bronze, but are composed of separate parts, wrought with the chisel, and afterward joined together. The hollows in the hind parts are filled with lead, which has assumed its shining reddish appearance. These parts are gilt: yet the gilding is nearly effaced, though, according to Buonarotti, the gold with which the ancients covered their bronze was to ours as six to one.

Of copper

These horses were supposed to be of copper, because this metal takes gilding better than bronze; and I have been enabled to verify the fact on a small piece weighing 40 grains, which was sent me. From this it appears, that the copper was not absolutely pure, as it contained a little tin; but the oxide of tin obtained from these 40 grs. amounted only to 0·35 of a grain; so that, when reduced to the metallic state, the proportion would be only 7 parts of tin to 993 of copper. This proportion is so small, it may be presumed to have been accidental.

with a very little tin.

Might not bronze be advantageously employed for utensils in our days?

In our days the use of iron and brass has singularly diminished that of bronze, which was so frequently employed by the ancients. It is now confined to cannons, bells, and statues*. But is it not desirable, that our copper vessels should be replaced by vessels of bronze or brass, as they are less liable to oxidation, and to injure the health? This question deserves to be solved by comparative experiments. What ought to induce us to examine this important question is, that the ancients employed only vessels of bronze in their kitchens and cellars in general, though they were well acquainted with the injurious qualities of oxide of copper taken internally. This oxide however they used externally for cleansing and healing wounds. According to Aristotle, wounds made with weapons of bronze were more easily cured than those made with weapons of iron.

Metal of the quadriga of Chios examined by Darcet.

In a note subjoined Mr. Darcet observes, that the metal of the horses of the Carrousel, taken as it is, yields copper, tin, lead, gold, and silver. If the surface be filed, so as to remove all the gilt part, nothing is found but copper, tin, and lead. If a piece perfectly free from cracks be taken, and thoroughly cleaned by the file, it yields copper and tin alone: but it is difficult to procure such pieces, for the copper is full of flaws, and the mixture of lead and tin, with which the horses were partly filled, has insinuated itself into every crack. On analysing some select pieces, he found copper 99·177, tin 0·823: but as sulphuric acid disturbed the transparency of the solution, he supposes a little lead was present, and that part of the tin might come from the alloy of tin and lead, which had covered the inside of the pieces he used.

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* Mortars for apothecaries and druggists too are commonly made of it. C.

He could not procure a piece well gilt, to examine in what way the gold was applied; but he observes that the brittleness of the metal seems to indicate, that quicksilver was employed.

III.

On the black Sand, or Menachanite, found on the Coasts of Liguria: by D. Viviani, *Prof. of Botany and Natural History in the Imperial Academy of Genoa*[*].

Situation of minerals to be studied.

THE situation of minerals becomes daily more important to be known, as it furnishes the geologist with knowledge of importance to the theory of the Earth, and the mineralogist with characters highly interesting with respect to that kind of association, which takes place between different minerals.

That of menachanite unknown.

The situation of the ferriferous oxide of titanium has not yet been studied in a satisfactory manner. There is even a variety of this species, known by the name of menachanite, which has hitherto been found only in the form of sand, and the situation of which is consequently unknown to the naturalist. The black sand found on the seashore between Pegli and Sestri, about four miles from Genoa, having been analysed by my learned colleague, prof. J. Mojon, was found to be true mechanite, of which it has all the mineralogical characters. Hitherto it had been found only on the seashore, where it never appeared but after great storms, which had confirmed the opinion of its submarine situation, and destroyed all hope of ascertaining it.

Supposed to be washed up by the sea,

but appears on the shore only after land floods,

I had visited the shore however several times between Pegli and Sestri, after pretty violent storms at sea unattended with rain, without finding any appearance of menachanite; on the contrary I found a stripe skirting the sea for a hundred paces or more, if the storm at sea had been accompanied with abundant rain, and particularly if the brooks of Sestri and Pegli had inundated this part of the shore, after washing the surface of the surrounding mountains.

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[*] Journal de Physique, vol. LXIX, p. 314.

therefore probably brought from the mountains.

This observation, frequently occurring, led me to suspect, that the black sand on this shore was furnished by the mountains, that pour their waters into the Varenna, as well as by other torrents running between Sestri and Pegli; that the conflict between these fresh water streams and the waves of the stormy sea directed against the shore effected a washing of the earth containing the mechanite; and that thus the mineral was deposited in the form of sand on the shore, while the earthy substances, being less heavy, remained some time longer diffused and suspended in the seawater.

Torrents traced to their sources in Liguria.

This conjecture inspired me with the hope of finding the native bed of the mechanite in the mountains above Sestri and Pegli, which form the streams of the Varenna and other little torrents, that flow across that shore. I determined therefore to trace these torrents to their sources, and at the same time ascertain the nature of the mountains.

The torrent of Varenna divides the territory of Sestri from that of Pegli. Though dry in summer, it is above a hundred yards broad at its mouth. It forms the edges of the plains that border it on both sides, and adds to them annually; but it frequently resumes its rights, and destroys in a day the work of ages.

Mountains from which they proceed.

The torrent comes from a chain of magnesian mountains, which shelter the fine plantations of Pegli from the north wind, and render its climate the mildest in all Liguria. To the right of this torrent, above Pegli, begins a system of mountains of micaceous schist, which runs sometimes into shining at others into micaschist. This system is continued to the south-west all along the Apennines, and forms the nucleus of these mountains in the western range to their junction with the maritime Alps. It is between Pegli and Sestri only that we can properly establish the commencement of this system; for the slaty schist, and secondary limestone, extend from the serpentine chain of Bracco, in the eastern range, as far as the Scoglio de St. Andrea, a distance of about eight and twenty miles, where they suddenly disappear, and are replaced by transition rocks, till we come to the micaceous schist*.

I sought in vain for the black sand in the bed of the Varenna. The pebbles with which it is loaded, and still more the fine sand and clay, which in the alluvions are last deposited on the bed of the torrent, do not allow it to be discovered.

I then directed my search to the slope of the mountains, both on the side of Pegli and on that of Sestri; to examine whether mechanite were one of their component parts.

Menachanite found at their feet.

My first discovery of menachanite was in the little wood Grimaldi, above Pegli, on the right of the Varenna, about 1 kil. [3278 feet] above the level of the sea, and about 100 met. [328 feet] from the place where the menachanite had hitherto been found†.

The night before I observed this a very heavy rain had fallen. The streams descending from the mountain had furrowed the ground in all directions, and let fall on the borders of the furrows the gravel they had washed down. On these borders I soon saw the black sand, which had every appearance of the menachanite on the shore of Pegli, and which was found on examination to be perfectly similar to it.

This sand traced to some distance.

I then traced this black sand to the beginning of the edge of the plain between the foot of the mountain and the sea; and no doubt should have been able to trace it to the shore, if the cultivation of this plain, and the successive deposits of the Varenna, which I have already mentioned, had not interrupted my search.

The mountain examined and described.

My next step was to examine with attention the mountain on the back of which I had just discovered the menachanite. It is formed of a micaceous schist, of a silvery gray colour, composed of thin laminæ so tender as to be broken by the nail, though they cannot be detached in large scales. These laminæ, in pieces fresh broken, have a silky lustre; and in some places they are as finely striated, as if they were formed of fibres united longitudinally. The latter characteristic and their colour disappear in pieces that have been exposed to the air; an ochrey yellow, a blackish tint, and a gray, mingling together, and destroying the primitive colour.

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* The different systems of mountains observed in the Apennines of Liguria, and every thing that concerns the natural history of this country, will be displayed in my Tours in the Apennines, which will soon appear.

† Here is evidently some mistake, either in the copy, or of the printer. I imagine it should have been 3278 feet from the shore, and at 328 feet elevation. C.

Particles of mica are seen glistening on some parts of the rock: but, beside these detached scales disseminated through the rock, this mineral exists in it in a state of extreme attenuation, for it produces the silvery lustre, which covers the surface of the stone as with a varnish.

An examination of this schist gave me no indication of mechanite, except the black colour, which sometimes spread through the interior of the rock, and indicated a kind of decomposition; but hitherto my observations furnished me with nothing beyond simple conjecture.

The formation of an artificial lake above the wood of Grimaldi, having required a perpendicular cut in the mountain, afforded me a favourable opportunity of pushing my inquiries farther. I then observed a kind of stratification in the mountain; but the strata are so disordered, that it is impossible to ascertain their general direction. Some veins of quartz run in the direction of the strata themselves. The black colour, which I had already observed extends into the substance of the schist, mixes with a yellow ochre, which fills the cavities produced by the decomposition of the stone. I examined these cavities with a lens in places where the black colour was deepest, and found that this colour was produced by a pulverulent substance, in shining, angular grains, attracted by the magnet, and in short exhibiting all the mineralogical characters of menachanite.

A cut in the mountains showed some parts of the rock in a state of decomposition and containing menachanite,

From the pieces of schist, which I had broken off from the mountain, I selected some, that exhibited throughout large spots of this black substance, mixed with the yellowish ochre I have described. These I powdered in a mortar; and from this powder the magnet separated several grains of menachanite perfectly resembling that on the shore of Sestri and Pegli.

perfectly resembling that found on the shore.

Thus I can no longer doubt, that menachanite, or the ferriferous oxide of titanium, enters into the composition of this micaceous schist, and occasions its black colour; and that it is these metallic grains, which, in consequence of the decomposition of the rock, are washed down by the waters, and thrown upon the shore after separation by washing.

This sand therefore a component part of a primitive rock.

From these observations we find, that the granular and sandy form, under which menachanite appears, is by no means occasioned, as has hitherto been supposed, by the action of the water washing it along. We have seen, that the menachanite, as soon as it is separated from its gangue, appears in the same granular and sandy form as on the shore of Sestri. The situation of this mineral therefore must be considered as one of the most ancient among metallic substances, since it is found among the component parts of a primitive rock; and appears not to have been in its original state even when this rock was formed.

Analogous to gold dust.

Some analogy may be perceived between these sandy grains of menachanite, and the gold dust, that was erroneously supposed to be separated from ores of this metal, and mixed with sand by the waters. But this gold dust has already been found in soil brought from some other place, which leaves us uncertain as to the period, when the gold was reduced to this form.

Other instances of sand among primitive rocks.

I see nothing to connect this singular situation of the menachanite with any geological epoch but the sandy depositions, and thin strata of sand, which the illustrious de Saussure has found interposed and almost mingled with the composition of primitive mountains. Menachanite, as well as these sands, may have been reduced to granular fragments in some more remote ages of our globe.

Whatever may be thought of these conjectnres, the situation of menachanite in the shining schist of the mountains of Pegli is established. Thus, since the torch of the natural sciences has begun to illumine the land of Liguria, the vicinity of Genoa has already made known the natural situation of two minerals, the variolite in mount Ramazzo*, and the menachanite, which forms the subject of this paper.

Variolite in mount Ramazzo.

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* Annales du Muséum.

IV.

IV.

Remarks on some properties of Nickel; by Professor TOURTE, *of Berlin*[*].

Needle of nickel.

THE preparation of a needle of nickel for the royal mineralogical cabinet at Berlin afforded me an opportunity of making a series of experiments on metallic nickel, which will not perhaps be uninteresting. The piece of nickel employed was fifteen inches long, seven lines broad, and a line and half thick. It was dull and rough, with here and there a metallic lustre. Its colour was similar to that of silver of twelve pennyweights heated red hot. The metal loses but little of its lustre in the air, and it takes a fine polish. This is best effected by smoothing the surface with a fine file, then rubbing it on a bluestone with water, and finishing the polish with tin putty and oil. Burnishing does not answer so well. When well polished, its colour is a mean between steel and platina. If heated redhot, the colour is changed to a grayish green, resembling antique bronze[†]. The intensity of colour increases in the oxide every time it is heated, and the nickel loses its lustre. Dilute nitric acid removes the oxide, and leaves the metal with some degree of brightness.

The metal described.

Burns in oxigen gas.

In oxigen gas the metal burns, and throws out sparks; but some care is requisite, to make this experiment succeed. When the metal, placed on redhot charcoal, is exposed to a stream of oxigen gas, it is converted into a grayish green oxide*.

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[*] Annales de Chimie, vol. LXXI, p. 103. Translated from Gehlen's Journal, by Mr. Vogel.

May be blued like steel.

[†] Nickel well polished is more liable to be oxided by the air than iron. But it may be blued over the fire like steel; becoming first of a light yellow, then a deep yellow, afterward a light violet blue, and lastly of a deep violet blue. If the temperature be raised, it becomes a blueish gray. The intensity of the colours is not equal to that in steel; yet I found this property in nickel very striking. The blueing requires a higher temperature for nickel than for iron. TOURTE.

Its specific gravity.

Richter gives the specific gravity of this metal cast at 8·279, and when hammered at 8·666: I do not know at what temperature. At 10° Reaum. [54·5° F.], and 27 in. 8 lines [29·4 in. Eng.] of the barometer, I found the specific gravity of nickel slightly hammered 8·402; and thoroughly hammered, of which the needle was composed, 8·932.

Its properties for working.

The difficlty of fusion of nickel is particularly favourable to its being wrought. It is ductile and tenacious, and may even be drawn into the slenderest wire. Soldering it succeeded with me but imperfectly; and I found the rods made by Richter not well united, so that it was difficult to find a uniform bar for a needle. The oxide that forms at a red heat is probably the cause of the imperfection of the soldering, for the parts overlapping each other may be separated without much resistance. When nickel has been exposed to a white heat, and cooled, it may be bent like lead, without breaking: nor does the oxide form a brittle coat, scaling off like that of iron when bent; it is rather pulverulent. The metal has but little hardness and elasticity; and on this account its tenacity and ductility are the greater. It may be filed, but it wears the file. To work it the files should be first soaked in oil. It is quickly heated by filing.

An excellent conductor of heat.

To appreciate its power of conducting heat, I made an experiment with a wire of nickel 7 inches long, 3 lines thick, and weighing an ounce and half. By its side I placed two similar wires, one of copper and the other of zinc. One end of each was inserted into a bit of wax, the other into an iron ball 2 inches in diameter. The wax at the end of the nickel melted first; those on the copper wire, and on the zinc, did not melt till afterward. In the course of my

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Attempt to burn nickel without success.

* Last winter I made some experiments on the combustion of nickel in oxigen gas, in the presence of Messrs. Baader, Ritter, Fuchs, and Herkel; but notwithstanding the fineness of the wire, and all the care I took, I could not succeed. It merely acquired a white heat. We then placed the wire in contact with a watchspring. The latter burned as usual, but the nickel only appeared inclined to burn. In fact its extremity was rounded to a globule, yet it was extinguished as soon as the spring was consumed. This sufficiently proves the difficult oxidability of the metal. GEHLEN.

operations I had frequently noticed the extraordinary manner in which nickel conducted heat, and this led me to make the above experiment.

Its magnetic property

The magnetic effects of nickel attracted my attention, and I availed myself of the present opportunity, to examine this remarkable phenomenon. Richter has said, that nickel alloyed with copper retains its magnetic property; and that arsenic, on the contrary, is the real destroyer of magnetism. I have no doubt, that arsenic may weaken the magnetic power of nickel, particularly when it is in considerable quantity: but I satisfied myself by my needle of arseniated nickel, that small portions of arsenic weaken it only slightly. Hence I shall never judge of the purity of nickel by its magnetic action.

not destroyed by small portions of arsenic,

but diminished by oxigen.

On the other hand I found, that oxigen diminished the magnetic property of nickel, so that there was a marked difference between surfaces more or less bright. The metal I used was divisible with regard to its magnetic property into two portions: the larger was magnetic plus; the smaller, which was about a fifth of the whole, was minus. Between the two was a point of indifference.

Its poles very different in extent.

Destroyed by heating,

I tried the action of heat on the magnetic power. After heating the nickel redhot, I found its action evidently weakened, but its poles were not changed. Heating it redhot a second time diminished its power still more; and after the sixth heating its magnetic property was completely destroyed. A similar effect took place with a powerful needle. I satisfied myself of the ease with which this metal acquires polarity, by placing a piece within the atmosphere of a magnet of moderate power, which acted at 3 inches distance. By this approximation I obtained the same poles as before, but of less intensity. A continued red heat caused the magnetic property to disappear again. A few blows with a wooden mallet reproduced some slight traces of it; and its action became stronger, after the nickel had been forged on the anvil.

and in part restored by forging.

Its polarity probably acquired.

It does not appear probable to me, that the metal, after being reduced, should acquire polarity without the application of a magnet. The conjectures of Richter too appear to me inconclusive, when he examined the magnetic power of nickel in grains by bringing near them a magnet, which would necessarily render these grains magnetic. It appears certain, that it is more difficult to deprive nickel of magnetism than iron. It would be interesting to science, to examine whether the variation and dip of a needle of nickel be the same with those of a needle of steel. I found the dip of a needle of steel similar to that of a needle of nickel touched by the magnet. They were both to the north.

Its dip the same as that of a steel needle.

V.

Abstract of a Paper on the Tenacity of ductile Metals, the Changes of Density in Lead by the Operation of Pressure, and the Action of distilled Water on this Metal, read to the Physical and Mathematical Class of the Institute, by GUYTON MORVEAU*.

Tenacity of metals differently stated.

THE author of this paper having observed in modern works of the highest estimation expressions of the tenacity of some metals very different from those he had deduced from his experiments, published in the 25th volume of the Ann. de Chim., deemed it of sufficient importance to institute a new examination, and to add at the same time the observations he has since collected for completing the synoptic table of the distinguishing properties of metals, which he had drawn up for his course of lectures at the imperial polytechnic school. Not that he thinks the maximum of this force of cohesion of metals should enter into the calculations of artists who employ them; for it is well known, that the accidental imperfections, which always more or less facilitate their rupture, oblige them to have recourse to larger dimensions; but it is not the less true, that the known ratios of their tenacity are of use to determine which should be preferred, and afford an important test of their purity, and the quality of the manufactured metal.

With regard to copper, platina, silver, gold, and iron, the author's experiments agreed with those of Dr. Thomson; but with respect to other metals he differs from him.

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* Abridged from the Annales de Chimie, vol. LXXI, p. 189.

Zinc deserving attention.

The examination of the tenacity of zinc appears the more to deserve attention, as it appears to be in contemplation, to substitute it for lead in covering roofs of houses; and as, on account of its great dilatability, it was considered by Smeaton particularly advantageous in the construction of compensations for timepieces.

Examined in different states.

Mr. Morveau has examined it in different states: he tried a bar of forged zinc purified by Mr. Vauquelin, sheet zinc from Limbourg given him by Mr. Descotils, and zinc laminated and drawn into wire by Messrs. Praire and Tournu. The expression of the resistance, which was the mean term of eight experiments, places it immediately after gold, as in the following table.

Table of the tenacity of different metals.

	kil.	lbs. avoir.
A wire of 2 millim. [0.787 of a line Eng.] in diameter of iron supported before it broke a weight of	249·659	549·250
Copper	137·399	302·278
Platina	124·690	274·320
Silver	85·062	187·137
Gold	68·216	150·753
Zinc	49·790	109·540
Tin	15·740	34·630
Lead (calculating by the dimensions at the point of rupture)	12·555	27·621
(calculating by the dimensions before it stretched)	5·623	12·371

Nickel.

The author would have included nickel in this table, now admitted into the class of ductile metals, and the tenacity of which Mr. Richter presumes must be very considerable; but the trials to which he subjected it gave only a tenacity of 47·67 kil. [104·875 lbs.] for a wire of the above diameter. It is true the piece he tried, which he had from Mr. Vauquelin, and which he considered as pure, did not exhibit in the process of flatting all the ductility, that Mr. Richter announced; whence he inferred, that we must wait for more decisive experiments, before its place in the table can be absolutely determined.

Two observations, that offered themselves to the author in the course of his inquiry, led him to some researches, that form important sections in his paper.

Density of lead diminished by flatting,

The first is the diminution of the specific gravity of lead by the process of flatting, confirmed by numerous experiments of Muschenbroeck, and which Dr. Thomson has related, at the same time confessing, that the cause still remains unknown. Mr. Morveau was particularly induced to clear up this anomaly, as in a paper in the 2d part of the Memoirs of the Institute for 1806 he had recorded a fact, which appears contradictory to this: four men were unable to ram home a cannon ball surrounded by a ring of lead.

unless it is compressed on all sides.

Having verified this diminution of the density of lead, and determined with precision the circumstances accompanying it, when extended under the hammer, passed between rollers, drawn out as wire, and struck with the engine; he perceived, that this effect took place only in proportion to the facility with which this metal softens, this occasioning the metal to escape from the pressure, even when struck in a collar; as was clearly shown by the quantity of the metal that rose at each stroke. He was resolved however to obtain a more direct proof, by stamping planchets of lead in a very strong collar, in which they were completely confined between two plates of iron. In this way their density was gradually increased from 11·358 to 11·388. Hence it is to be concluded, that lead, when confined in a place from which it cannot issue out as if it were fluid, is susceptible, like other ductile metals, of a degree of compression, which brings its particles nearer together, and increases its specific gravity.

Lead readily dissolved by pure water,

The second phenomenon, to which the author's attention was called, was the very speedy action of water on lead; the distilled water, in which it was suspended from the hydrostatic balance, soon acquiring a milky aspect, and a white flocculent sediment being at length deposited in it.

He satisfied himself by a series of experiments, which he gives with their results, that distilled water acts on lead spontaneously, and without the assistance of agitation: that this action takes place even on lead revived from the muriate: that it occurs with water distilled in glass vessels, a circuma circumstance that excludes all galvanic influence: that it ceases, when this water has been deprived of air by boiling, or by exposure to the vacuum of an airpump: that it stops, when the air the water was capable of furnishing is exhausted: that it recommences, when air is restored to the water: that the presence of any neutral salt, as the sulphates, nitrates, muriates, in ever so small a quantity, as for instance 0·002 of sulphate of lime, is sufficient to obstruct this action: and that to this is owing the preservation of lead without alteration in the water of the Seine, in well-water, &c., whether in open or in covered vessels. Hence this metal may be considered as one of the most accurate tests of the purity of water, provided the water contain no salt with excess of acid.

as long as the water contains any air:

but not in water containing any neutral salt.

With regard to the nature of the product of this action, there is a manifest oxidation of the metal, but without any decomposition of the water; different from that of iron, or of zinc, which takes place in common water as well as in distilled water, and even in that which is totally deprived of air. It is not a simple oxide however: its lightness; its flocculent form; its silvery lustre; the crystalline points perceptible on the surface of the sediment; the state of litharge of a golden yellow, which it assumes when heated; the rapidity with which the approach of a hidrosulphuret gives it the appearance of a galæna in shining scales; and lastly the drops of water, which the heat of the sun extricates from it after it has been long dried in the open air, with the little effervescence it produces in acids; lead the author to suppose, that this product is of the nature of a hydrate.

The product is probably a hydrate.

VI.

Improved mode of preparing Phosphorus Bottles. In a Letter from a Correspondent.

SHOULD Mr. Nicholson think the following observations worthy a place in his valuable Journal, they perhaps may tend to lessen a difficulty occasionally experienced by individuals, in the prosecution of a favourite study.

Improved mode of preparing phosphorus bottles.

It probably may be acceptable to G. O., vol. XXV, p. 189, to be informed of a method of preparing a phosphorus bottle, which is in a considerable degree free from the inconvenience attending those prepared according to the method made use of by him.

Phosphorus, cut into small pieces and mixed with quick lime in powder, answers the purpose very well. The phosphorus should be carefully dried by filtering paper, a thin slice being cut may be divided into as many pieces as can expeditiously be done, and each piece introduced into a small bottle, with as much lime, as will surround it. Lime slacked in the air, and submitted to a strong red heat, in a black-lead crucible for twenty minutes, is in a good state for the purpose.

The bottle, when full, may be exposed corked, to the radiant heat of a fire, till some of the pieces of phosphorus have assumed an orange tint, it will then be ready for immediate use. But the heating is not absolutely necessary if the bottle is not wanted for immediate use, and it will continue longer in a serviceable state.

It is almost superfluous to observe, in using the bottle the mouth should be closed with the finger as soon as the match is withdrawn.

Bottles thus prepared may be used frequently during four or five months.

I have been in the habit of preparing a bottle by this method, at the conclusion of winter, for the purpose of lighting a lamp furnace during the summer months, when I had not convenient access to a fire. A narrow quarter ounce bottle has generally continued serviceable four or five months, though very frequently used.

Lancaster, 17*th March*, 1810. P.

VII.

Remarks on Professor Leslie's Doctrine of Radiant Heat; with Experiments to show, that Caloric can pass through transparent Media without heating them. By J. D. MAYCOCK, *Esq.*

(Concluded from page 78.*)*

I Proceed, Sir, to offer for your consideration a few observations on Mr. Leslie's experiments, and on my own.

If,

Caloric passes through transparent media without raising their temperature:

If, as Mr. Leslie affirms, caloric never passes through a medium without first raising its temperature, blackened glass, used as a skreen, should affect the thermometer in a greater degree than plain glass: for by blackening the surfaces of the glass both their absorbing and radiating powers are increased: but we find by Exp. 2d and 4th, that the thermometric rise with plain glass is more than twelve times higher than with blackened glass. Exp. 5th and 8th, however, afford yet more satisfactory evidence of the calorific rays passing through diaphanous bodies independently of heating their substance. Another argument in opposition to Mr. Leslie's opinions, if more were requisite, might be drawn from the results given in Exp. 9th. If his position were well founded, since the effect with plain glass is to the effect with painted glass as 7 to 5; and since, when the hot flask is removed, the painted glass produces an effect as 15; the plain glass should produce an effect as 21. But when the primary source of heat is removed, the effect of the plain glass is to the effect of the painted glass only as 13 to 15.

It is an indubitable fact, that, when a transparent sheet of glass is brought near to a hot body, it will have its temperature augmented; but the fairest inductions from my experiments warrant us in concluding, that the use of the thermometric fluid is not solely, or principally, referrible to this secondary source of heat.

but not through opaque media.

Mr. Leslie's position, however, holds perfectly true with respect to the action of opaque skreens. It is rendered probable by every experiment, in which they were employed; and is satisfactorily proved by Exp. 6th, 7th, and 9th. This leads me to notice the results of Exp. 2nd and 3rd. When an opaque skreen is interposed between the thermometer and the hot body, one surface of the skreen absorbs caloric from the hot body, the other surface radiates to the thermometer. The most considerable effect, therefore, should be produced on the thermometer when a skreen is used, the surfaces of which are best adapted for absorbing and for radiating, and the substance of which is the best conductor of caloric. In the two former qualifications the painted tin and the painted glass are on a par; in the latter the painted tin has the decided advantage: yet in Exp. 2nd the thermometer indicated a higher temperature than in Exp. 3rd. I consider this unexpected result as depending on the great difficulty of so completely painting the surfaces of the glass and of the tin as entirely to destroy the transparency of the one, and the polish of the other.

In some instances heat passes through opaque as readily as through transparent media.

We learn from Exp. 12th, that, when a single mirror is employed, and boiling water is the source of heat, caloric does not pass more readily through glass than through an opaque body; and Mr. Leslie's experiments show, that, as the skreen is removed from the hot body, the effect on the thermometer diminishes, and at last entirely disappears. But even these results admit of explanation on principles very different from those assumed by Mr. Leslie.

Attempt to explain this.

We well know, that diaphanous media always intercept a number of the calorific rays; and may therefore fairly conclude, that such media offer a resistance to the passage of caloric. It seems to me reasonable to suppose, that it is by their momentum, that the rays of caloric overcome this resistance; whence I infer, that a perpendicular direction is that most favourable for a ray of caloric to impinge on the surface of glass that it may pass through its substance; and conclude, that the greater is the angle of incidence of a ray, the less will it be able to overcome the resistance offered to its passage. When a single mirror is employed comparatively few calorific rays impinge on the surface of the glass in such a direction as enables them to overcome the resistance; too few sensibly to affect the thermometer. Of the rays not transmitted some will be reflected, some will be absorbed by the skreen; its temperature will be raised, and it will consequently radiate caloric to the thermometer. When therefore we operate with one mirror, the thermometer seems to derive its temperature immediately from the interposed skreen, whether it be plain or painted glass. But when we employ two mirrors, a number of rays are made to fall perpendicularly on the surface of the skreen; they are transmitted by the plain glass; they are intercepted by the painted glass. Hence, in Experiment 9th, with plain glass the thermometer rose as 7, with painted glass only as 5.

Yet we know, that the painted glass is more heated than the plain, for the hot flask being removed, the painted glass skreen occasioned a farther rise as 15 to plain glass as 13.

Theory of radiant heat.

The higher the temperature the fewer rays will be detained in a diaphanous medium.

The best theory of the radiation of caloric supposes it to arise from the repulsive force, which prevails between the particles of this fluid, from which property it is projected in right lines from every body in which it is accumulated. Now as diaphanous bodies admit some rays of caloric to pass through their substance, and intercept others, it seems a probable conjecture, that all the rays of caloric have not the same degree of velocity; and that, the higher the temperature of a body is, the greater number of rays will it project with such a velocity, as fits them for passing through a dense medium. We might therefore with much reason suppose, that a certain number of calorific rays projected from a burning candle, and falling perpendicularly, or nearly so, on the surface of a sheet of glass, would penetrate its substance: that from the boiling sulphuric acid fewer would have the requisite degree of velocity; and from the boiling water still fewer. On such an hypothesis we can readily explain, why with a burning candle the thermometer is brought to a higher temperature than the plain glass screen; why with a flask of boiling sulphuric acid and water the thermometer and screen are at nearly the same temperature; and why with a flask of boiling water the screen is hotter than the thermometer.

Mr. Leslie's experiment with ice not conclusive.

Mr. Leslie, having convinced himself, that the calorific ray does not pass through a sheet of ice, considers this fact as an additional evidence of the truth of his position.—But it surely cannot be admitted as such. Experiment proves, that water in its fluid state stops a considerable number of the calorific rays emanated from the sun, which we have reason to believe travel with greater velocity than those projected from bodies artificially heated. Besides, when water freezes its particles take on a crystallized arrangement; the solid mass becomes porous, whence it is less adapted for the transmission of the calorific ray, and will no doubt be, from the same cause, less pervious to radiant caloric. But the experiment was made with a single reflector, and it has been already granted, that with this apparatus the thermometer is immediately affected by the screen, which however affords no support to Mr. Leslie's general position.

His experimentum crucis examined.

As Mr. Leslie considers his 10th as an experimentum crucis, which establishes his theory beyond the power of contradiction; I would occupy a moment of your time in observing, that although we admit, that, when a single reflector is used, the thermometer derives its temperature immediately from the skreen, and that the compound skreen is heated much sooner when the glass surface is opposed to the hot body than when the metallic surface is; yet we by no means find ourselves constrained to adopt Mr. Leslie's hypothesis for explaining the radiation of caloric; as it rests not on these facts, but on the general conclusion deduced from them—That caloric passes through no medium independently of raising its temperature.—A conclusion, which does not necessarily follow from any of the facts or reasonings to be found in the experimental inquiry, and which, from my own observations, I feel myself authorized to reject.

Made with two reflectors it might have turned out differently.

I am inclined to think, that, had Mr. Leslie's experimentum crucis been made with two reflectors, and had he noted the time occupied by it, he would have obtained results similar to those which he has related p. 35 of his book; and they would have admitted of easy and satisfactory explanation on the established principles, which Mr. Leslie considers as wholly unfounded, but to which, on account of their simplicity and apparent truth, I confess myself attached.

Heat reflected only from the surface on which it impinges.

I pointed out in experiment 10th and 11th, that the thermometer is differently affected as the blackened or the plain surface of a sheet of glass is opposed to the hot body. Yet in both experiments the matter of the skreen is the same; and the radiating and absorbing powers are nearly equal. I think the phenomenon may be explained in the following manner. Mr. Leslie's 4th experiment shows, that the calorific rays are reflected only from the surface on which they impinge; the degree of reflection from a glass mirror being always the same, whether the back of the mirror was silvered, preserved clean, or ground with sand or emery. It seems therefore a fair conclusion, that the calorific ray, (projected from bodies artificially heated) after passing through glass cannot be made to return again by any substance placed on the opposite surface of the glass. In experiment 10th the black surface absorbs as much caloric as from its nature it is qualified to do: the skreen has its temperature raised a certain number of degrees, and becomes a source of heat to the thermometer. But in experiment 11th some of the calorific rays are immediately absorbed by the glass surface; others pass through the substance of the glass, but are intercepted by the painted surface: and not being returned through the substance of the glass, they must assist in raising the temperature of the whole skreen, which will consequently become hotter in this than in the former experiment.

Radiant caloric penetrates some media without heating them.

I trust, Sir, the experiments I have detailed will be sufficient to satisfy you, that radiant caloric, under favourable circumstances, penetrates glass, and perhaps other diaphanous media, independently of raising their temperature. I shall be glad to hear of their being repeated on a more extensive and varied plan; and regret the want of leisure, which for the present prevents my farther prosecuting this interesting subject.

I have the honour to be, Sir,

Your most obedient servant,

No. 99, *Nicolson Street, Edinburgh,* J. D. MAYCOCK.
April the 5th, 1810.

VIII.

On the Acrid Principle of Horseradish: by Mr. EINHOF*.

Horseradish root distilled.

TWO pounds of the root of horseradish, dug up in the month of October, were rasped, and the pulp distilled on a water-a waterbath. An oily fluid passed over, and in the neck of the retort were streaks of oil. When five ounces of fluid had come over, the apparatus was unluted, and the fluid put into a phial.

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* Annales de Chim. vol. LXX, p. 185. Translated from Gehlen's Journal.

Product.

Essential oil.

This product had a very pungent smell of horseradish, so as to occasion considerable pain in the nose of a person smelling to it. It was turbid. After standing some time, about the quantity of ten drops of an essential oil, of a pale yellow, and of the consistence of oil of cinnamon, was found at the bottom of the phial. Its smell was intolerable, but perfectly similar to that of horseradish root fresh scraped. Its taste was at first sweetish, like that of oil of cinnamon; but it left a burning acrid sensation behind, and the parts of the tongue and lips touched with it became very red and inflamed. A drop of this oil on a pane of glass is very quickly volatilized at the temperature of 12° R. [59° F.], and fills the room with a strong smell of horseradish. It falls to the bottom of water, but mixes with it on shaking, and forms a milky liquor, like that obtained by distillation. Alcohol dissolves it easily and completely.

Water.

Contains sulphur.

The distilled fluid is not altered by any reagent, except the nitrate of silver, and acetate of lead. The first changes it brown, and throws down a black precipitate: the second produces a brownish precipitate, indicating the presence of sulphur, which Gutret and Tingry had likewise found in horseradish. The water saturated with essential oil comports itself in the same manner.

Pungency soon evaporates unless in close vessels.

The distilled water, or that saturated with oil, if exposed to the air, soon loses its pungent smell, and retains only that of turnips: but in close vessels it preserves its strength for years. Having left a portion of the distilled water, with a few drops of the oil at the bottom, standing in a phial for a twelvemonth, in a cool place, the oil had disappeared, but some small shining needles of a silvery white were formed. These I collected, but the quantity was so small, I could not make an accurate analysis of them; so that I am not certain, whether they were benzoic or camphoric acid. These crystals when dried had a strong smell of horseradish, and irritated the throat. They dissolved slowly and incompletely in alcohol. In a spoon over the flame of a candle they melted, and diffused a strong smell of horseradish, succeeded by a smell of peppermint, and lastly of camphor. As the heat increased they were entirely dissipated.

The horseradish remaining in the retort, being mixed with water, yielded a few more drops of oil, and a distilled water, with all the properties mentioned above.

Deep buried mould analysed.

Smell of horseradish.

I must observe, that, having subjected to analysis some mould, dug up from a depth of twenty feet, I obtained an ammoniacal water, hidrogen gas, and carbonic acid. This compound gas lost its fetid smell, and after a time smelled like horseradish. The water remaining in the jars in which this gas had been collected threw down a black precipitate with solution of silver, which appears to indicate the presence of sulphur.

IX.

Analysis of the Galvanic Pile. By J. A. De Luc, Esq. *F.R.S.*

Part I.

Works on meteorology.

Electricity and galvanism identical.

IN January 1806, I had the honour of presenting to the Royal Society two works, connected with each other by the common object of *meteorology*, published at Paris in 1803 and 1804, under the titles of *Introduction à la Physique terrestre par les Fluides expansibles*, & *Traité Elémentaire sur le Fluide électro-galvanique*. In the latter of these works I had proved, by direct experiments, that it was the *electric fluid* itself, which acted in the *galvanic pile*; but that it underwent there a *modification*, which made it produce, with an excessively small quantity, some effects, which else it could not produce but with a very great quantity set in motion by the discharge of the Leyden vial, and even of batteries. This fundamental proposition was established in my work by a particular mode of analysis of the effects of the *pile*, which however other avocations obliged me to publish unfinished, but expressing the hope, that some other experimental philosopher would take it up and follow it; which has not been the case.

Chemical agencies of electricity.

In 1807, I saw in Part I of the Phil. Transactions a *Bakerian* lecture of Mr. Humph. Davy, *on some chemical agencies of electricity*, which revived my attention to this subject. The very ingenious and interesting experiments which distinguish that paper are well known, and my praises would add nothing to those, which it has deservedly received: but a theory was there introduced, which I considered as involving the *electric phenomena* in the thickest veil, and this was my motive for resuming the above experiments. Mr. Davy supposes a *positive* and a *negative* energy, as belonging to distinct *substances*, constituting a class of *general causes*; and in p. 39, after having specified some of the *bodies* to which he attributes these different *agencies*, he concludes thus: "*In the present state of our* "*knowledge* it would be useless to attempt to speculate on "the *remote cause* of the *electrical energy*, or the reason "why different bodies, after being brought into contact, "should be found differently *electrified*; its relation to *che-* "*mical effects* is however sufficiently evident: may it not "be *identical* with it, and an *essential property* of matter?"

Mr. Davy's theory.

Occult qualities.

Tutored in Bacon's school, I have found in the long course of my study of natural phenomena the profound wisdom of the following passage, in his immortal work *De Augmentis Scientiarum*, lib. III, cap. V. Speaking there de occultis & specificis proprietatibus, which he considers as belonging to a sort of *magia*, the offspring of false *metaphysics*, he says: "Primum enim intellectum "humanum in soporem conjicit, canendo *proprietates spe-* "*cificas* & *virtutes occultas*, & tanquam cœlitus demissas, & "per traditionum susurros solummodo perdiscendas: unde "homines ad *veras causas* eruendas non amplius excitantur "& evigilant, sed in hujusmodi otiosis & credulis opinioni- "bus acquiescunt; deinde vero innumera commenta, & "qualia quis optaret maxime, instar somniorum, insinu- "ant*."

There

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* "First it throws the human understanding into sleep, lulling it with sounds of *specific qualities* and *occult virtues*, as if they came down

from

Should not be applied to the natural phenomena of electricity,

There is not in the whole field of natural philosophy an object, to which the application of this warning of a true philosopher can be of greater importance, than that under consideration. Such *formulæ* as Mr. Davy employs in lieu of *causes* may appear satisfactory within the narrow limits of the experiments which he describes; but all the spontaneous phenomena of our globe, in which the *electric fluid* is concerned, are thus thrown into the back ground, and never noticed. There is evidently, however, no greater agent in these phenomena, than the *electric fluid*, which Mr. Davy never considers as a *substance*; he speaks only of *electricity*, of *electric energies*, which are empty words in themselves, when supposed to imply the idea of *causes*; while all the meteorological phenomena proclaim a *fluid*, the *chemical affinities* of which, already manifested, open the road to the most important inquiries.

which are evidences of a distinct substance.

Analysis of the effects of the pile led to the following

This consideration was my principal motive in resuming the analysis above mentioned: I have said, that I had not pursued it so far as I saw it possible, but far enough to oppose the erroneous conclusions, into which Mr. Davy's obscure principles had led him, with respect to the *mode of action* of the *galvanic pile*; and continuing that analysis by an uninterrupted series of experiments, I arrived at the following propositions, as conclusions of the whole.

general propositions,

1. *Positive* and *negative* in electricity are mere *relations* to a certain *standard*, concerning the distributions of the *electric fluid* among bodies, with which *relations* no *chemical* effects are connected.

2. The immediate effect of the combination of two proper metals in the pile, and of their repetition, is to accumulate a certain quantity of *electric fluid* on one half of the length of the pile, which the other half loses.

3. When the two extremities of the pile are connected together by conducting substances, the above property of the pile produces a *circulation* of the *electric fluid*, passing

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from Heaven, and could not be learned but by the whispers of tradition: whence men are no more solicitous in the discovery of *real causes*; they acquiesce in these idle opinions, and hence numberless gratuitous comments, which, like those on dreams, every man may apply as he wishes.

constantly from the side on which it tends to accumulate, to the other side, where it compensates the deficiency tending to recur.

4. This *circulation* of the *fluid* produced by a proper association of metals can exist in the same quantity, with the same number of pairs of metals, without either *chemical* effects in the *circuit*, or the *shock*.

5. For the production of these phenomena it is necessary, that a *liquid*, being introduced between the two *metals*, a *calcination* be produced on them. In this case, the *electric fluid* circulating through the *metals* is *modified*, but its *modification* is different according to the *liquid*: with *pure water*, there are *chemical* effects in the *circuit*, but no *shock* is felt; the latter requires, that the circulation be produced by an *acid*.

Deduced from experiments.

Such were the conclusions deduced from the experiments related in the paper, which I delivered to the Royal Society the 30th of May, 1808; but as this and a following paper have not been admitted into the Phil. Transactions, I shall resume here the same course of experiments and deductions, but by more direct steps, being enabled to do so by the progress of my researches from that time. For this purpose I shall begin by explaining the reason of the plan which I have followed in this inquiry from its beginning in 1800.

Reason of the plan pursued.

If we consider a mounted pile without taking notice of its extremities, we see a repeated association of three constituent parts, two *metals* and wet *cloth* or *paper*; which I shall name *silver*, *zinc*, and *wet cloth*, as they were in my fundamental experiments. By considering this composition of the *pile*, the leading circumstances for me were, first, that its *effects* increase with the *number* of the repetitions of the three constituent parts; secondly, that the whole together forms a *conductive* column. These circumstances concur to point out some cause, which accumulates at the extremities of the pile the opposite effects produced in a certain *ternary association* of the component parts: but which is that *association*? In reflecting on this question I was convinced, that, till it was decided, the *mode of action* of the *pile* would remain entirely unknown.

The

Arrangement of the pile in ternary groups.

The *pile* may be considered as divided into *ternary groups* under three different aspects.—1. *Zinc* and *silver* and *wet cloth* placed *between* them.—2. *Zinc* and *silver* in *mutual* contact with the *wet cloth* on the side of *zinc*.—3. *Zinc* and *silver*, still in *mutual* contact, but the *wet cloth* on the side of *silver*. On this, two questions arose in my mind; first, to which of these *ternary* groups are owing the *accumulation* of the *electric fluid* on one side of the *pile*, and *increase* of its *deficiency* on the other, which become greater with the *number* of these groups? Second, what is the cause, that so small a quantity of *electric fluid*, set in motion by the pile, produces effects, which require so great a quantity of the same *fluid*, when put in motion by any other means hitherto known?

Separation of these to discover which is active.

On the first queſtion, supposing the *conducting* faculty of the *pile* to be the cause of the accumulation of the opposite effects produced on the *electric fluid*, I concluded, that small metallic *conductors* placed between the really *efficient groups* would not disturb the effects: but that, if these *conductors* were so placed as to produce separate *ternary* associations different from that to which the effects are attached, the latter would cease. Consequently that by effecting the three different *divisions* of the *pile* by small *conductors*, I could not fail to discover the *efficient groups;* a knowledge which might lead to resolve the second question.

Necessary instruments.

When I proceeded in this plan, I soon found, that no deep analysis of the operations of the *pile* could have been obtained without these admirable instruments, the *gold-leaf* electroscope of Mr. Bennet, and the *condenser* of Sig. Volta. So minute are the quantities of *electric fluid* necessary to be observed in the course of these experiments.

Apparatus described.

The necessity of having gold-leaf electroscopes directly connected with each extremity of the *pile*, determined the form of the apparatus which I used for these researches, which is represented in Plate III, fig. 1: it consists of two similar *frames*, which may be used separately for other purposes; but for these experiments, they are fixed on the same board, and they form a *pile* divided into two columns A and B. The *frame* of each *column* is composed of three

glass

Apparatus described

glass rods, covered with insulating varnish, and the assemblage of *groups* contained in each *frame* rests at the bottom on small insulating pillars. The space within the glass rods (except a small part of it at the bottom, in which a section of plates is represented) is, in the *figure*, indicated by only a dotted line; because it varies in height, in the course of the experiments, according to the number of the *groups* and their arrangement. The order of succession in the associated plates of the different *metals* being inverse in the opposite *columns*, these are connected together at their lower parts by a brass slip *d*, which becomes the *middle point* of the *pile*, and its *extremities* are thus transported to the tops of the *columns*.

The upper part of each frame is constructed in the usual manner; the three glass rods passing through a piece of wood, where they are wedged, in order to give to a screw, which passes through the piece of wood, the power to press on the groups at the different heights which they attain, in different stages of the experiments. With each column is connected a gold-leaf electroscope, movable upon a wooden rod: from its top projects a brass spring, which presses on the top of the column, in order to secure a real contact. When the gold leaves diverge, and it is wanted to know whether the divergence is *positive* or *negative*, the electroscope is lifted up only so far as to separate it from the pile, and is then tried in the usual manner.

With respect to *chemical* effects in the *circuit*, I shall confine myself to those which were first observed in *glass tubes* filled with *water*, at the opposite extremities of which enter wires, the latter serving to connect the water with the extremities of the pile. I made these experiments with different kinds of wire, but in the main course of them I used *brass* wires: thus, while *inflammable air* proceeded from one of them, *calcination* took place on the other: these are the only effects which will be considered in the following experiments. For a purpose which I shall explain, I placed two of these *tubes* in the *circuit*, connected together in their lower parts by a *brass ring*, as may be seen at *c*, fig. 1.

In the description of the experiments, the *chemical* effects will be noticed at the end of each wire in the glass tubes; for which purpose, these ends are indicated by the numbers 1, 2, 3, 4. During these *chemical* effects, the *electric states* of three points in the *circuit* will also be investigated; namely, *a*, the point of connexion between the extremity A of the pile and one of the glass tubes; *c*, the ring which connects the two tubes; and *b*, the point of connexion between the second tube and the extremity B of the pile.

Apparatus described.

I proceeded at first in a very complicated manner, described in my work, to the intended *dissections* of the pile, by small *conductors* interposed between different *ternary* arrangements of its three component parts; but in resuming these experiments, I used a more simple method, by forming *tripods* with two pieces of small brass wire, soldered together with soft solder in the shape of a T, as represented at fig. 3, Pl. IV, the three extremities of the wires, turned downwards, form three feet *a*, *a*, *a*, one eighth of an inch high, which thus produce the communication of the *tripods* with the group on which they rest by only three small points; and the wires themselves being a little bent downwards, the group above rests also on the *tripod* by three points *b*, *b*, *b*.

In all the experiments, the same order of succession of the *metals* was preserved in both *columns*, notwithstanding their different dissections by the *tripods*: in A, the column begins at the bottom by a *zinc* plate, thus terminating at the top by a *silver* plate; and in B, a *silver* plate begins the column at the bottom, which thus ends at the top by a *zinc* plate. I mention this circumstance on account of a question concerning the denomination of the extremities of the pile by the *metals*, which will occur in the course of the experiments.

From what has been said above of the three different *ternary* groups that may be composed in the same succession of the three component parts of the pile, fig. 2, which represents the different states of the latter in these experiments, will be easily understood; but it must be recurred to, when each experiment is related. No. 1 represents a portion of the *continuous pile*, which is the same from one end

Apparatus described.

end to the other. No. 2, is a *first dissection* of the pile by the *tripods*, in which the latter divide it into *ternary groups* composed of the two *metals* and the *wet cloth* between them: this also prevails from one end to the other of the pile. No. 3 represents the *second dissection* of the pile, in which the *tripods* divide *ternary groups* composed of the two *metals* in mutual contact, and the *wet cloth* in contact with *zinc* only; but this division not being complete at the extremities of the pile, this No. is represented in two parts; one being the top of col. A, terminated by a *silver* plate alone, and the other, the top of col. B, terminated by a *zinc* plate with *wet cloth*. The case is the same in No. 4, shewing the *third dissection* of the pile, in which the *wet cloth* is in contact with *silver*: this representation also consists of two parts, one being the top of col. A, terminated by a *silver* plate with *wet cloth*, and the other the top of col. B, terminated by a *zinc* plate alone. In order to leave place for the letters *z* and *s* near the plates, those of *silver* are represented of the same thickness as the *zinc* plates, though they were not so, as I am going to explain.

In order that the frames of my columns might contain the greatest number possible of *groups* when divided by the *tripods*, I used very thin *silver* plates, these being very little acted upon by the liquid during this kind of experiments, each of which lasted only till the different effects were observed; but it is not the same with *zinc*, the surface of which is soon calcined, and must be often restored to the metallic state; so that, on account of the thickness of the *zinc* plates, each column could contain only 38 groups with the tripods, forming a pile of 76 groups, the plates of which were 1·6 inch diameter; and this remained the number of groups in all the following experiments, as far as relates to this part.

Lastly, as the different *liquids*, with which the pieces of cloth may be imbibed, produce different effects, I determined to follow this first course of experiments with two different liquids, namely, *pure water*, and a strong *solution of marine salt*, in order to compare their effects. Thus I made two *sets* of the same experiments, in both of which

I began by a *continuous pile* of 76 groups, and continued by their three different *dissections* with the *tripods*.

I. *Set of Experiments, wherein the pieces of cloth were imbibed with pure water.*

Continuous pile.

Experiments with the continuous pile.

This pile being mounted, and remaining insulated, with only the electroscopes applied to its extremities, their indications, expressed, as they will always be, by the measure of the divergence of the gold leaves in *decimals* of an inch, were at that moment:

A, 0·0——B, 0·4 *negat.*

Exp. 1.

Exp. 1. I tried the *shock* in the surest manner, which is to hold in each hand, thoroughly wet, pieces of metal, for which I used silver spoons, with a drop of water at their extremity: but I did not feel any *shock*.

Exp. 2.

Exp. 2. With the two *glass tubes* filled with water, and applied to the pile as represented in fig. 1.

1. No *electric sign* remained at the extremities; a proof, as I shall have an opportunity of showing, that the *circulation* of the *electric* fluid was produced through the water of the glass tubes.
2. A small stream of *calx* descended from the wire 1.
3. *Inflammable air* ascended from wire 2.
4. *Calx* descended from wire 3.
5. *Inflammable air* ascended from wire 4.

The next point which I had an intention to examine was, whether there were any possibility to discover the *course* followed by the *electric fluid* in its *circulation*; whether from A to B, or the contrary way. This brought to my mind what happens to a stream of water when it meets with a narrow channel in its course; it rises more or less at the entrance, according to the degree of *retardation* which it undergoes, and is proportionably lower below the issue. I thought therefore, that, if there were any *retardation* in the course of the *electric fluid* passing through the water of the glass tubes, there would be some *accumulation* of the fluid at its *entrance*, and *deficiency* at its *issue*, which might be discovered by connecting with a condenser the three points *a*, *c*, *b*, in fig. 1. I tried a condenser, the plates of which were separated by a piece of *silk*; but in these very delicate experiments, it produced deception; the *silk* retaining some electrisation from one experiment to the other. I was therefore obliged to use a condenser acting by the mere *distance* of the plates, and I constructed one on a new plan, which answers many purposes. The under, or *receiving* plate is horizontally fixed on an insulating pillar. By means of a brass wire with an insulating handle, I connect successively, each time during 20 seconds, the above three points with the under plate of the condenser, with which is connected a gold-leaf electroscope. During the time only that the upper plate is concentrically situate over the former, it is in connection with the ground; after which, by a proper mechanism, this upper plate is made to retire, by revolving on an insulating axis, and I observe the divergence then produced in the electroscope.

Condenser of electricity on a new plan.

I must still mention another circumstance concerning this class of experiments. When the extremities of the insulated pile are only connected with their electroscopes, the indications of the latter differ according to external circumstances, which I shall not introduce here, saying only, that sometimes there are indications on both sides, *positive* at A and *negative* at B, at various correspondent degrees; sometimes also there is only an indication at A, then *positive;* and at other times, as in the above case, the divergence is only at B, and *negative.* But if A be placed in communication with the ground, the whole electric *difference* between the extremities is expressed at B with the *negative* sign; and on the contrary, if B communicates with the ground, the whole *difference* is expressed at A with the *positive* sign. I must also remark here, that there are two different *standards*, very distinctly and judiciously defined in Sig. Volta's system, to which these *comparative* expressions refer. The two *standards* frequently agree, but often also they differ, from a cause which I shall explain on another occasion. When the *divergence* of balls or gold leaves in the electroscopes is considered alone, the *standard* of *plus* and *minus* is only the *actual electric state* of the *ambient air;*

Indications of the electroscope differs according to circumstances.

Two different standards.

air; but if we want to know the *electric state* of insulated bodies, the *standard* becomes the *actual state* of the *ground* in that respect. When therefore a body is in communication with the *ground*, it is neither *plus* nor *minus*, and I shall call it *neutral*; meaning in the *electric state* of the *ground*.

Neutral.

In the insulated pile, when its extremities are connected together by the glass tubes, if there be some *retardation* of the *electric fluid* in pervading the *water*, it hardly can be perceived, because this is a middle point between the *positive* and *negative* states; but if one of the extremities be placed in communication with the ground, there may remain a *residuum* of the *current* retarded, discoverable by the condenser. This is the reason of the manner in which I have made the observations at the three points *a, c, b*: I first observed them during the insulation of the pile; then in placing alternately the extremities A and B in communication with the ground, and this I did by the contact of my finger during the communication with the condenser.

Exp. 3.

Exp. 3. While the *chemical* processes were going on in the glass tubes, I applied the condenser to the three points *a, c, b*, in the manner above described, and the results were as follows:

	Without contact.	*With contact of* B.	*With contact of* A.
a	*pos.* 0·2	*pos.* 0·3	0·0
c	0·0	*pos.* 0·2	*neg.* 0·2
b	*neg.* 0·1	0·0	*neg.* 0·4

Course of the fluid.

This experiment clearly points out the *direction* of the course of the *electric fluid*, as I expected would happen, if it underwent some *retardation* in pervading the *water* of the tubes: it certainly *enters* the *water* from the side *a*, since a *positive residuum* exists at *a* and *c*, when the *contact* is on B: it *goes out* and returns to the pile by the side *b*, since a *negative residuum* exists at *c* and *b*, when the *contact* is on A; and this is the reason why the middle point *c* passes through three different states; it is *neutral*, when no acceleration is produced in the natural course of the *fluid* by the contact of either of the extremities; but if the course is accelerated by more of the *fluid* coming from the ground to the side B, the *middle point* becomes *positive*; and if the acceleration proceeds from some part of the *fluid* flowing into the ground by the side A, the *middle point* becomes *negative*.

Chemical effects of the pile not connected with positive and negative energies.

This experiment shows also how unfounded is the idea, that the *chemical* effects here taken as an example are connected with *positive* and *negative* energies. During the course of the above observations with the condenser, the two *chemical* effects continued sensibly in the same degree at the extremities of the same *wires*, though these underwent changes in their *electric states*. At wire 1, the *electric state* of which is the same as that of the point *a*, *calx* is produced whether it be *positive* or *neutral*: at wire 4, the *electric state* of which is the same as at point *b*, *inflammable air* is produced whether it be *negative* or *neutral*: but the phenomena are still more decisive at the wires 2 and 3, the *electric states* of which are similar to each other and also to that of the point *c*. Now here we see *inflammable air* at wire 2, and *calx* at wire 3, whether they be *positive*, *negative*, or *neutral*. Whence results finally, that the real connexions of these *chemical* effects with the course of the *electric fluid* are the following: *calx* is produced at the wires by which the fluid *enters* the *water*, and *inflammable air* at those by which it *goes out*.

Exp. in which the pile being rendered alternately negative and positive by connexion with an electric machine, the chemical effects were the same.

The same experiments were already related in my work presented to the Royal Society in January 1806, and repeated in my first paper; but in the former, sect. 544, I related another experiment still more striking, which I had made at Berlin with Prof. Erman, who as well as myself was then much employed in galvanic experiments, and possessed a very powerful electric machine. He had at one time an insulated pile of 200 groups of *zinc* and *silver* the size of crown pieces, newly cleaned, by which the usual effects of the production of *calx* and *inflammable air* in the water of a glass tube were going on very rapidly. We connected this pile with the *rubber* of the machine, the *prime conductor* being in connexion with the ground; which was a most powerful way of producing the *negative* state in the *whole* pile, so that it drew sparks from our hands at a great distance; however we perceived no change in the glass tube, the *chemical* effects went on with the same rapidity: and the increase of the quantity of the fluid, by connecting the pile with the *prime conductor* of the machine, which made the pile emit spontaneous flashes, did not sensibly increase these *chemical* effects. Not having then to contend with the idea that *negative* and *positive* states were properties of any substance, I concluded only from the above experiments, that our strongest means of depriving the pile of *electric fluid* left always enough of the latter for the *circulation* produced by the property of the former, so small is the quantity thus set in motion; and that the increase of its absolute quantity did not sensibly increase that which *circulates* in the pile.

First dissection of the pile.

Exp. 4. Wet cloth in contact with each metal, & the metals separated.

This second kind of *pile* is represented at fig. 2, No. 2: the *tripods*, in this *dissection*, are placed between the two *metals*, thus separating *groups* in which these *metals* have *between* them *wet cloth*.

Exp. 4. The experiments made on the *continuous pile* having been detailed, I shall only assemble under this head the comparative effects of the two *piles*.

1. This new mode of experiments having been made immediately after the former, the *free pile* (I mean without connexion either between its extremities or with the ground) had the same *electric* indications.

2. No *shock* was perceived.

3. When the *glass tubes* were applied, the same *chemical* effects were produced in them, at the same *wires;* they only began later, and were a little smaller.

4. The condenser applied to the three points *a*, *c*, *b*, manifested, in the same circumstances, the same *residua* of *electric signs*.

At first it might be thought, that this *dissection* indicates the *efficient groups* for both *electric* and *chemical* effects; but the following experiment will show, that the *efficient groups* for these two functions are different.

Second

Second dissection of the pile.

Exp. 5. The metals in contact, the wet cloth touching the zinc only.

In this *pile*, represented at fig. 2, No. 3, the *tripods* separate *groups* composed of the two *metals* in *mutual contact*, and the *wet cloth* in contact with only the *zinc* plates.

Exp. 5.—1. The *free pile* had the same *electric indications.*

2. There was no *shock* produced.

3. The *glass tubes* being applied, the *electric indications* ceased absolutely at the extremities of the *pile*, a proof that the *circulation* of the *electric fluid* was produced; however no *chemical* effect appeared in the *water*.

4. The condenser being applied to the points *a*, *c*, *b*, no *retardation* was manifested in the course of the fluid.

This experiment begins to show, that the *causes* of production of *electric* and *chemical* effects, by the *pile*, are different; since here the former continue, but the latter are not produced.

Third dissection of the pile.

Exp. 6. Metals in contact, the wet cloth touching the zinc only.

This *dissection* is represented at fig. 2, No. 4: the *tripods* here divide the *pile* into *groups* composed of the two *metals* in mutual contact, but the *wet cloth* in contact only with *silver*.

Exp. 6. Cessation of every effect: no *electric* sign at the extremity of the *free pile*; no *chemical* effect in the *glass tubes* when connected with it; no *electric* sign at the points *a*, *c*, *b*.

Before I come to the conclusions, concerning the *mode of action* of the *pile*, which may be derived from this first *set* of *experiments*, in which the pieces of *cloth* were imbibed with only *pure water*, and no *shock* was produced, I shall relate the second *set* of experiments, in which a strong *solution of marine salt* was used; as all the results, except with respect to the *shock*, remaining the same, their union will render the conclusions more certain.

II. *Set*

II. *Set of Experiments: wherein the pieces of Cloth were imbibed with a strong solution of Marine Salt.*

Exp. with strong solution of muriate of soda.

Continuous Pile.

Exp 7.

Exp. 7. In order to have a point of comparison of the respective *electric effects* of the two *liquids,* I first mounted the *continuous pile* of 76 *groups* with *pure water* in the *cloth,* and observed the *electroscopes* at its extremities: they were this day A, 0·1 *pos.*; B, 0·3 *negat.* I then imbibed the *cloth* with the *solution of marine salt,* and the electroscopes had the same indications.

Exp. 8.

Exp. 8. Concerning the *shock.*

1. In placing my fingers, when *dry,* on the extremities of the pile, I felt no *shock.*

2. My fingers being *wet,* I felt a small *shock.*

3. Taking then the *silver spoons,* as above indicated, I had a *shock* up to the elbow.

4. Whenever I withdrew one of the spoons, on bringing it again into contact, I felt a new *shock*; but when, after having felt a *shock,* I kept both spoons steady on the extremities, all sensation ceased. This is an important fact, to which I shall refer hereafter.

Exp. 9.

Exp. 9. I applied the two *glass tubes* as in *Exp.* 2.

1. Every circumstance, with respect to *chemical effects* in the *water,* was the same, except that these effects appeared sooner, and were stronger; and every visible *electric sign* ceased at the extremities of the pile as in the other experiments.

2. I tried the *shock* with the spoons; it was not quite so strong as before the *glass tubes* were placed. I fixed also both tubes on the extremities of the pile; every sensation ceased, but observing then the *chemical effects* in the water of the tubes, I saw, by the quantity of *inflammable air* emitted, that they were reduced to about the half of their intensity: whence it appears, that my body was a *conductor* of the same nature as *water.*

3. Taking off the ring which connected the *glass tubes* by the hooks of their lower wires, and placing the angle of the handle of one spoon in one of these hooks, when I

touched with the second spoon the other wire, I felt a strong *shock*: but when I succeeded (which is difficult) in placing the angle of the other spoon steady in the other hook, all sensation ceased, and the *chemical effects* went on in the *tubes*.

Exp. 10. *Exp.* 10. Repetition of *Exp.* 3; or of the application of the condenser to the points *a*, *c*, *b* in the *circuit*.

	Without contact.	*With contact of* B.	*With contact of* A.
a	*pos.* 0·15	*pos.* the gold leaf struck	0·0
c	0· 0	*pos.* 0·6	*neg.* 0·5
b	*neg.* 0·45	0·0	*neg.* the gold leaf struck.

These results are of the same *nature* as in the experiment with *pure water*, but the *quantities* are much increased, showing a greater *retardation* of the *electric fluid* in its course; a circumstance which will be noticed hereafter.

First Dissection of the Pile.

Exp. 11. *Exp.* 11. I shall assemble here all the points correspondent to those of the above experiments on the *continuous pile*.

1. The *same electric signs* at the extremities of the *free pile*.
2. The same effects concerning the *shock*.
3. The same *chemical effects* in the *glass tubes*, only smaller.
4. The same *residua* of *electric signs* at the points *a*, *c*, *b*.

Second Dissection of the Pile.

Exp. 12. *Exp.* 12. Assembling also here the comparative results.

1. The same *electric signs* at the extremities of the *free pile*.
2. No *shock*.
3. No *chemical effect* in the *glass tubes*, though the *electric signs* had ceased.
4. No *residua* of these *signs* at the points *a*, *c*, *b*.

Third

Third Dissection of the Pile.

Exp. 13. Cessation of every effect with this *dissection,* as in *Exp.* 6.

Exp. 13.

Such are the leading experiments with respect to the *mode of action* of the *pile*; but before I come to their general conclusions, I must return to the particular circumstance belonging to *Exp.* 5 and 12, namely, that at the same time that no *chemical effects* were produced in the *water* of the *glass tubes*, no *residua* of *electric signs* remained perceptible in the points *a*, *c*, *b* of the *circuit.* This circumstance might appear favourable to Mr. Davy's idea on the *mode of action* of the *pile,* thus expressed at p. 45 of his paper above mentioned.

Circumstance apparently favourable to Mr. Davy's theory

"In the voltaic pile of zinc, copper, and a solution of "muriate of soda, in what is called its condition of *electric* "*tension*, the communicating plates of *copper* and *zinc* are "in opposite electrical states. And with respect to *elec*"*tricities* of such very *low intensities*, water is an *insulating* "body: every copper plate consequently produces, by "*induction*, an increase of *positive electricity* upon the "opposite zinc plate; and every *zinc* plate an increase of "*negative electricity* on the opposite copper plate; and the "*intensity* increases with the number, and the *quantity* "with the *extent* of the *series.* When a communication is "made between the two *extremities*, the opposite *electri*"*cities* tend to *annihilate* each other; and if the *fluid me*"*dium* could be a substance incapable of *decomposition*, "the *equilibrium*, there is every reason to believe, would "be restored, and the *motion* of *electricity* cease."

I shall not consider for the present that system in itself, but only on account of the connexion which it may appear to have with the above 5 and 12 experiments. For as at the same time that, in these experiments, the process called *decomposition* of *water* had ceased in the *glass tubes*, no *residua* of the *electric fluid* were perceptible by the condenser in any part of the *circuit*; it might be supposed, according to Mr. Davy's idea, that the *motion* of the *electric fluid* had really *ceased.* But the following experiments will show, that the cause of no *residua* appearing in the

differently explained.

cases here referred to is, that the *circulation* of the fluid in these cases becomes *too rapid*, to be discovered by that *test*.

Exp. 14.

Exp. 14. After *Exp.* 12, I took off the *glass tubes* from the pile.

1. The electroscopes at its extremities, then free, indicated, A, 0·0, B, *neg.* 0·4.

2. I produced the communication between the two extremities by a *brass* conductor; every *electric sign* ceased, even to the *condenser*.

3. I substituted a *slip* of *deal*, which I had left a long while in *water*, in order that *water* itself might become the *conductor*: it produced the same effect as *brass*, every *electric sign* disappeared.

4. I left this *slip* in the same situation, in order to observe the effects in the progress of *drying*. The *water* had already disappeared at the surface, and no *electric sign* was perceived, even by the *condenser*: after some time, the *condenser* manifested these signs, *positive* at A, and *negative* at B; at last the same signs became visible in the electroscopes of the *pile*.

Electric fluid circulates through the pile.

I think it now evident in general, that an actual *circulation* of the *electric fluid* is produced, through the *pile* and any *conducting* substance which connects its extremities; that this *circulation* is naturally so *rapid* when the *electric fluid* does not undergo any *retardation* in the *conductors*, that its course cannot be manifested even by the condenser; and that when it is manifested by *electric signs*, these are produced by some *retardation*.

Its course may be retarded by the nature of the conductors, or modification of the fluid: the first a test of the insulating property of varnishes.

Now: a *retardation* in the course of the *electric fluid* set in motion by the *pile* may proceed, either from the *nature* of the *conductors*, as in the above experiment; or from some *modification*, which the *electric fluid* itself undergoes in pervading the *pile*. The former cause of *retardation* of the *electric fluid*, manifested by the preceding experiment, affords, as I shall show in another paper, the best and easiest method of trying the *insulating* faculty of the different *varnishes* laid on *glass* for supports of electric apparatusses; an essential object in practical electricity; but the latter, namely, the different *modifications* which the *electric fluid* undergoes by pervading different *piles*, will become an important object in the course of the conclusions from the above experiments, to which I now come.

Electric and chemical effects produced by different groups.

Electric group;

The first of these conclusions will concern the fundamental *mode of action* of the *pile*, in its two different effects, *electric* and *chemical*. When I devised the different *dissections* of the *pile* by small *metallic conductors*, I expected, that it would lead to the discovery of the *efficient groups ;* this has happened, but in an unexpected manner; the *efficient groups* are not the same for the *electric* and *chemical* effects. For *electric effects*, the *efficient groups* consist simply of the *binary* associations of the two *metals ;* each group being separated from the next by a *conducting substance nonmetallic*.

Chemical group.

For *chemical effects*, the *efficient groups* are *ternary*; they are composed of the two *metals*, having *between* them a *liquid* in contact with both, which here is in the *wet cloth*.

Both in piles 1 and 2 :

If *Fig*. 2 is considered with that view, it will confirm these determinations. In the *piles*, No. 1 and 2, which produce both effects, the two kinds of *groups* exist. The *binary groups* of metals, to which are owing the *electric effects*, are formed, in No. 1, by the immediate contact of the two *metals*; and in No. 2, they are produced by the interposition of the brass *tripods*; and in both piles, these *binary groups* are separated by a *nonmetallic* conducting substance, which is the *wet cloth*. The condition of *chemical effects* in the *circuit*, which is the *ternary groups* above defined, exist also in these two piles, as in each the *wet cloth* is placed *between* the two *metals*, in contact with *both*.

electric only in 3 :

But the case is different in the piles No. 3 and 4. In the former indeed, the condition of a *motion* of the *electric fluid* exists, namely, the *binary groups* of *metals*, separated by the *nonmetallic* conducting substance; and the *electric effects* continue : but the condition for *chemical effects* in the *circuit* is wanting; the *wet cloth* is in contact only with *zinc* and not with silver; in a word, it is not *between* the two *metals*, which is the condition.

neither in 4.

Lastly, the pile No. 4, which produces neither *electric effects*, nor *chemical effects* in the *circuit*, is deprived of the conditions of both. The *electric effects* are not produced, because *zinc* is between two metals which have the same relation with it in this respect; *silver* on one side, and the *brass* of the tripod on the other side. The condition for *chemical effects* in the *circuit* is also wanting; as the *wet cloth* is in contact only with *silver*.

These conclusions with respect to the determined difference between the two kinds of *groups*, to which I was first led by the above experiments, will be hereafter confirmed by more precise phenomena, shown by new experiments directed to that purpose; but I must first remove an apparent contradiction between these experiments, and what had been determined by other experimental philosophers on the *electric states* of *zinc* and *copper* when in mutual contact.

Apparent contradiction;

either metal seeming in some cases positive, in others negative;

It may be seen in the construction of the four different *piles* represented in *Fig.* 2, that the succession of *zinc* and *silver* plates continues the same in all, throughout the whole length; from which arrangement, the termination of the extremity A is by a *silver* plate, and that of the extremity B by a *zinc* plate; at the same time it has been seen in all the experiments made with these different *piles*, that the extremity A is the *positive*, and the extremity B the *negative*. This seems to imply, that the *silver* side is the *positive*, and the *zinc* side, the *negative*; whereas by other experiments, *silver* is *negative*, and *zinc positive*, when they are connected together. Such opposite conclusions from facts equally certain much embarrassed me, till by the study of what is represented in *Fig.* 2, and the conclusions to which it led me concerning the two different *efficient groups*, the riddle was solved in the manner that I am going to explain; which at the same time will be a confirmation of what has been determined above, with respect to the *groups*.

explained.

The condition of *electric effects* in the pile is *absolutely* this: the *two metals*, either in immediate *contact*, or connected together by a *metallic* conductor; these *groups* being separated by a *non metallic* conducting substance, here the *wet cloth*: wherever therefore one of the *metals* is not in this determined connexion with the other, it does not contribute to the *electric effect* of the pile, but is an extraneous body. Now, if notice be taken of all the *piles* represented in *Fig.* 2, it will be seen, that the last piece of *wet cloth*, at the extremity of each of them, leaves *above it* one of the *metals* unconnected with the other, so that the former therefore is a mere *conductor* to the *electroscope*. Thus the *efficient groups* for *electric effects* terminate at and *under* that *piece of cloth*; and it may be seen in the *figure*, that in this manner *zinc* terminates the extremity A, which is *positive*, and *silver* the extremity B, which is *negative*; and thus, not only the two experiments are reconciled, but thereby the definition of these *groups* in the *pile* is confirmed.

Without oxidation of the plates no shock produced.

Another phenomenon manifested in these experiments deserved a farther investigation, in order to avoid any mistake; as it leads to important conclusions. It has been seen, that, when the pieces of cloth were imbibed with *pure water* only, though *chemical effects* were produced in the *glass tubes*, there was no *shock*; but that this effect was produced, when the *calcination* of the *metals* in the pile was effected by the *acid* of *marine salt*. It was therefore important to know, whether this particular cause of *calcination* were an accidental, or necessary condition of the *shock*; and I thought of a way to decide it, which was, to substitute for *zinc* a metal, which, becoming *positive* with *silver*, and thus capable of producing a *motion* of the *electric fluid*, should be *calcinable* by the *acid* of *marine salt*, but not by *pure water*; I found *pewter* to be such a metal, and I made the following experiments; first by my condenser so constructed, that I could readily try the effect of small piles, placed on a *conducting* pillar, lifted up by a string against a brass ball projecting from the *receiving* plate, and which descends by its own weight when the string is relaxed.

Exp. 15. Pewter and silver.

Exp. 15. I tried in this manner a *pile* of 20 groups, *pewter* and *silver*, separated by cloth imbibed with *pure water*, and I found, that the *pewter* side affected the condenser *positively* and the *silver* side *negatively* in sufficient degrees, to give hope, that, by increasing the number of *groups*, immediate *electric signs* would be produced in the electroscopes at the extremities of the pile.

Exp. 16.

Exp. 16. I procured thin *pewter* plates the size of my *silver* plates, and I increased the number of these *binary groups*, separated by pieces of cloth imbibed with *pure water*, till I had sufficient *electric signs* at the extremities: this was by 100 *groups* in each column, forming a *pile* of 200 *groups*.

1. I observed and noted the divergences in the electroscopes, *positive* at A, the *pewter* side, and *negative* at B, the *silver* side.

2. No *shock* was felt.

3. The *glass tubes* being applied, though the *electric signs* ceased at the extremities, no *chemical effect* was produced in the *water* of the tubes.

4. The condenser applied to the usual points *a*, *c*, *b*, no *residua* of *electric signs* appeared.

5. Dismounting the pile, I did not perceive any impression of the *water* on the *pewter* plates.

Exp. 17. *Exp.* 17. I mounted a pile with a sufficient number of *groups* formed of *zinc*, *silver*, and *cloth* imbibed with the *solution of marine salt*, to produce at its extremities the same *electric signs* as the above pile; which I obtained by 25 groups in each column, 50 in the whole, and the following were the results.

1. I felt the *shock*.

2. The *glass tubes* being applied, the *electric signs* disappeared at the extremities, and the usual *chemical effects* were produced in the *water*.

3. I applied the condenser to the points *a*, *c*, *b*, and found very sensible *residua*.

Exp. 18. *Exp.* 18. I mounted again the pile of 200 groups *pewter* and *silver*, but I imbibed the cloth with the *solution of marine salt*.

1. The same *electric signs* were produced at the extremities.

2. I felt the *shock*.

3. The glass tubes being applied, the *electric signs* disappeared at the extremities, and the *chemical effects* were produced at the usual points in the *water* of the tubes.

4. Then also, the condenser being applied to the points *a*, *c*, *b*, sensible *residua* of *electric signs* were found in the usual order.

5. *Lastly*, having dismounted the pile, I found on the *pewter* plates many spots produced by *calcination*.

These experiments, different from those of the former set, having clearly decided the above question concerning the condition of the *shock*, the whole together ascertains the following facts.

General conclusions.

1. When there is no *calcination* produced on the *metals* of the pile, though the *electric fluid* be put in *motion*, no *chemical effect* is produced in the *water* of the tubes.

2. When that *calcination* is produced by *pure water*, though these *chemical effects* take place, there is no *shock*; the latter requiring the *calcination* to be produced by an *acid*.

3. When the *electric fluid*, in pervading the pile, is rendered capable of producing either one, or both of these effects, its course is *retarded* through the *water* of the glass tubes, and more in the latter case.

The last of these facts leads to the following conclusion, When the *electric fluid* pervades a pile wherein the *calcination* of some *metal* is going on, the *fluid* itself undergoes some *modification*, which is the cause of the concomitance of these phenomena, a *retardation* of its course through the *water*, and the production, with a very small quantity, of *effects* which it cannot produce but with an incomparably greater quantity, when set in motion by any other known means. But before I come to this object, it is necessary to ascertain an essential point, which so far I have only mentioned, namely, that the *motion* of the *electric fluid* in the pile does not depend on a separation of the *binary groups* of *metals* by a liquid, or a *wet body*; but requires only that the separation be produced by the best *nonmetallic conducting substance*; and with the leading experiments concerning this object I shall conclude this *first Part* of the *analysis*.

Presence of a fluid not necessary.

Exp. 19.

Exp. 19. I mounted the pile of 76 *groups* of *zinc* and *silver*; but instead of *wet cloth* to separate them, I used new *cloth*, which had stood some time in my room, my *hygrometer* being at about 40°.

1. *Electric signs* were produced at the extremities of the pile, but weaker than when the pieces of *cloth* were *wet*.

2. The *glass tubes* being applied, these *signs* ceased, but there was no appearance of *chemical effects* in the *water* of the tubes.

Judging that the diminution of the quantity of *electric fluid* in motion proceeded from a want of *conducting* faculty in the *wool* itself, and aware, that no *metallic* substance could be substituted for obtaining a better *conductor*, I undertook a long series of experiments, by forming piles of 20 groups *zinc* and *silver*, separated by all the substances of the *vegetable* and *animal* kinds that I could devise, applying these groups to the condenser. Of these experiments, however, I shall only mention the practical result, which was, that among all these bodies, *writing paper* was one of the best for the intended purpose, at the same time that it is the most easy to manage; and I made the following experiment.

Writing paper one of the best substances for separating the metals.

Exp. 20.

Exp. 20. I mounted again the pile of 76 groups *zinc* and *silver*, and separated them with pieces of *writing paper*.

1. I found a great increase in the *electric signs* at the extremities of the pile, comparatively with the *cloth*.

2. These *signs* ceased when the glass tubes were applied, but still no *chemical effect* was produced in the *water* of the tubes.

This experiment opened before me a new and extensive field, in which I have ever since travelled, as will be seen in the *second Part* of this analysis, and in a following paper.

X.

Hints on the Subject of Animal Secretions. By EVERARD HOME, *Esq. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry*[*].

Animal secretions perhaps

THE brilliant discoveries of Mr, Davy on the powers of electricity in producing chemical changes suggested to me the

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[*] Philos. Transact. for 1809, p. 385.

Dr. Wollaston's observations, inserted in the Philosophical Magazine ,were published after this paper had been laid before the Society.

I was

Produced by elecricity.

the idea, that the animal secretions may be produced by the same means. To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this Society, the object of which is the improvement of Animal Chemistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking. It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves.

Galvanic battery in the torpedo and electrical eel

The Voltaic battery is met with in the torpedo and electrical eel; and although it is given only as a means of catching their prey, and defending themselves, and therefore not

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I was led to the present investigation, while preparing my lectures on the Huntereau Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. William Brande to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir Joseph Banks, and stated, that I should bring them forward in my lectures: and at that time Dr Young's Syllabus was not published, and Dr Wollaston's opinions were unknown to me.

Dr. Berzelius, Professor of Chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced.

"Trace all the nerves leading to any secretory organ in a living ani-
" mal, and divide them, being careful to injure the blood-vessels
" and the structure of the organ itself, as little as may be: notwith-
" standing the continued circulation of the blood, the organ will as lit-
" tle secrete its usual fluid, as an eye deprived of its nerve can see, or
" a muscle the nerve of which has been divided can move. We may
" therefore easily conceive, that any trifling alteration in the nerves
" of a gland may materially affect its secretion, the supply of blood
" being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secrét mode of acting, when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy, while this paper was in the press.

immediately

abounds with nerves.

immediately applicable to the present inquiry, yet it furnishes two important facts, one, that a Voltaic battery can be formed in a living animal, the other, that nerves are essentially necessary for its management; for in these fish the nerves connected with the electrical organs exceed those that go to all other parts of the fish, in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy, to adapt them to receive and preserve a small electrical power.

Nerves and muscles form such a battery.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and communicating electricity, is proved by the well known experiment of taking the two hind legs of a vivacious frog, immediately after they are cut off; laying bare the crural nerves; applying one of these to the exposed muscles of the other limb; and then, when the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

Circumstances in the structure of the nerves not applicable to sensation.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and the uses of which have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs; the ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles; and the course of the nerves of the viscera, which keep up a connexion among themselves in so many different ways.

Blood vessels of the secretory organs do not account for their actions.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

Experiments

With a view to determine how far any changes could be produced on the action of electricity on the blood.

produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

Experiment 1. Middle of January, 1809.

Blood drawn and exposed to electricity.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been drawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed, as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood, which had surrounded the negative wire, was of a deep red colour, and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment 2. 8th of February, 1809.

Blood exposed to electricity in a vein.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it in the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the mesocolon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connection between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at that part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long continued electrical action, the serum only was employed in the following experiments.

Experiment 3. 10th of March, 1809.

Serum exposed to a high electrical power,

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment 4, 14th of April, 1809,

to a low power,

Was undertaken to ascertain the effect of a low power; a battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

Experiment 5. 6th of May, 1809.

and to a very low power.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates very weakly charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

Results.

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form, with a less degree, it is separated in a fluid form.

From these facts the following queries are proposed.

General conclusions.

1st. That such decomposition of the blood by electricity may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose, and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and

iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, while the production of acid and alkali, effected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the Society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

XI.

On the Saccholactic Acid, and its Conversion into Succinic: by Mr. TROMMSDORFF[*].

Process for procuring saccholactic acid.

THE process I employed for procuring saccholactic acid differs from that described by Scheele in requiring a smaller proportion of nitric acid. Into a large glass retort put one part of sugar of milk finely powdered, and three parts of nitric acid of the specific gravity of 1·28. A receiver being adapted to the retort, heat is applied, till the liquid boils. It is then removed from the fire; and, as soon as the ebullition and the evolution of gas have entirely ceased, it is placed on the fire again. When the liquid, which is kept boiling, begins to grow thick and turbid, the retort is taken from the fire, and suffered to cool. The white powder is then separated from the supernatant liquid, which is evaporated slowly, after adding to it the nitric acid, that had passed over into the receiver. In this way a considerable additional portion of saccholactic acid is formed, which is mixed with the other. The whole of the pulverulent acid is shaken repeatedly in a vessel of cold water, and separated by filtration. In this way from 16 ounces of sugar of milk four ounces of saccholactic acid are obtained, and from the mother water about two ounces of oxalic acid.

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[*] Abridged from Annales de Chim. vol. LXXI, p. 79 Extracted from Trommsdorff's Pharmaceutical Journal by Vogel.

A. 1.

Subjected to dry distillation,

A. 1. Two hundred gram. [3088 grs.] of saccholactic acid were introduced into a glass retort, to which a tubulated receiver was adapted, and from this a tube proceeded to the pneumato-chemical apparatus. The retort being placed on the naked fire, the saccholactic acid began to swell, grew black, and soon entered into fusion. First a white liquid passed into the receiver, then a yellow, accompanied with a heavy white vapour, and followed by a small quantity of empyreumatic oil. Carbonic acid gas was evolved, with a little carburetted hidrogen gas. The mixture of these gasses had a peculiar aromatic smell, by no means disagreeable. At the end of the distillation, about 60 gram. [926 grs.] of a transparent brown liquid were found in the receiver, and a yellowish crystallization. Neither the gaseous nor liquid products ever furnished any nitrogen. In the retort was a light coal, of a lustre almost metallic, which afforded some traces of lime when incinerated.

Coal

treated with nitric acid & dissolved by soda.

This same coal was treated repeatedly with boiling nitric acid. Being then heated with caustic soda, it dissolved in it, and produced a blackish brown liquid. Nitric acid threw down a deep brown precipitate from this solution. As the coal had considerably increased in weight, the author supposes he had formed the oxided coal of Proust.

Liquid in the receiver.

2. The brown distilled liquid was poured off from the crystals, and mixed with water, which separated from it a black empyreumatic oil: it reddened infusion of litmus; and had a pungent empyreumatic smell, but not ammoniacal. Being saturated with soda, neither prussic acid nor ammonia was evolved from it by heat. The liquid, being evaporated to dryness, was in part acetate of soda; but the base was probably saturated by some other acid also. The neutral solution was precipitated by a great number of metallic and other salts; as the nitrates of silver, mercury, copper, and lead, the muriates of iron and barytes, the sulphate of manganese, and the acetate of lime.

Properties of the sublimed acid.

3. The crystallized mass sublimed to the top of the retort had the following properties.

a. Heated in a silver spoon over the flame of a candle, it melted, and evaporated entirely, leaving scarcely a trace of charcoal. The vapour was inflammable.

b. Alcohol dissolves it without heat. After the evaporation of the alcohol, bundles of acicular crystals were left. If the matter be heated to fusion, and then left to cool, it forms a radiated crystallization, covered on the surface with small white needles.

c. Cold water dissolves this substance with difficulty, but it is easily dissolved in four times its weight of hot water.

d. The taste of this aqueous solution is perceptibly sour, and very different from benzoic acid. It has besides no analogy with the benzoic acid.

e. The aqueous solution was accurately saturated with soda; and a solution of muriate of iron at a maximum formed in it a dirty brownish red precipitate.

f. By way of comparative experiment, a solution of succinate of soda was poured into a solution of muriate of iron. The appearance was the same, except that the colour of the precipitate was less dirty, which may be attributed to some empyreumatic oil having remained in the former acid.

Similar to succinic acid.

All the results that had been obtained indicate a great analogy with succinic acid; but for more certainty it was purified, by washing with water the most perfect crystals, which were then dissolved in alcohol, and evaporated to dryness. Very pure succinic acid was employed comparatively with these.

Both gave the same results on exposure to heat,

a. Ten parts of the purified acid were exposed to a gentle heat in a phial over a lamp. When the acid was melted, a white smoke arose, which was deposited on the sides of the phial in fascicular flocks of a snowy whiteness. The phial being removed from the lamp, fine crystals an inch long sublimed in cooling. A little coal was left at the bottom, which when incinerated was found to contain potash.

aa. Ten parts of crystallized succinic acid, being treated in the same manner, exhibited exactly the same phenomena; and at the end of the process the two phials could not be distinguished from each other.

saturation with soda,

b. One part of the acid was accurately saturated with soda: and the same was done with succinic acid and soda. The quantity of water in these two salts with base of soda was the same.

c. The saturated solution did not precipitate acetate of lime, sulphate of manganese, nitrate of copper, acetate of lead, or nitrate of lead. The succinate of soda exhibited the same phenomena.

examination with various tests,

d. It precipitated the nitrate of barytes white, nitrate of nickel applegreen, sulphate of cobalt peachblossom colour, nitrate of mercury white, muriate of iron at a maximum brown. The succinate of soda comported itself in every instance in the same manner.

e. Three parts of nitric acid were evaporated from one of this acid. It was not decomposed, being only rendered whiter and more pure.

and treatment with nitric acid.

Succinic acid treated with nitric exhibited the same results.

To demonstrate the difference between this acid and the benzoic, I saturated twenty parts with soda, and dissolved the salt in 120 parts of water. Muriatic acid poured into it did not render it in the least turbid, but next day a solid crystallization was formed.

It differed from the benzoic acid,

The benzoic acid was saturated and dissolved precisely in the same manner. The first drop of muriatic acid began to separate the benzoic in a curdy form, and a larger quantity converted the whole liquid into a light bulky mass.

From these experiments it follows, that the acid separated from the products of the saccholactic by distillation is not the benzoic.

This acid is volatile, and crystallizable; it cannot therefore be compared with the acetic, oxalic, suberic, malic, citric, or tartaric. Neither has it any analogy with the prussic, uric, or sebacic. There remain then only the gallic, benzoic, mellitic, moroxylic, pyrotartaric of Rose, and succinic, to which it can be analogous.

and from all others,

It does not resemble the gallic, for it does not precipitate iron black, and when combined with soda acts neither on copper nor on lead. The benzoic acid is much less soluble in water, and the benzoate of soda precipitates almost all metallic salts. The mellitic acid comports itself very differently, according to Klaproth, with metallic solutions. The same may be said of the moroxylic and pyrotartaric. But except the succinic.

But to the succinic acid it is perfectly similar in taste, solubility, &c.

The liquid in the receiver examined.

A brown liquid, that passed over into the receiver with the sublimed acid, still remains to be examined. When it is saturated with soda, and slowly evaporated, a reddish brown saline mass is obtained. Part of this being mixed with concentrated sulphuric acid, no smell of acetic acid was emitted. The remainder of this neutral mass being dissolved in boiling water and filtered, a thick, black, empyreumatic oil remained on the filter. The filtered liquid was precipitated by acetate of lead, and this precipitate was decomposed by sulphuric acid. The liquid separated from the sulphate of lead had the following properties.

Properties of the acid it contained.

1. It precipitates the acetate of lead white.

2. By saturation with carbonate of potash, its colour is rendered deeper.

3. The neutral solution forms with muriate of lime, after some time, a precipitate, that has a saline aspect.

4. With nitrate of barytes it gives a slight precipitate, soluble in nitric acid:

5. With the nitrate of silver, or of mercury, a copious precipitate:

6. And with the nitrate of copper a dirty green precipitate:

The pyrotartaric.

These experiments seem to show, that the acid liquid contains the pyrotartaric acid of Rose.

Recapitulation.

General conclusions.

The saccholactic acid appears to contain carbon, hidrogen, and oxigen; but no nitrogen, for during its distillation in the dry way neither ammonia nor prussic acid is formed.

The saccholactic acid is decomposed by dry distillation, and yields succinic acid, pyrotartaric acid contaminated with an empyreumatic oil, a little acetic acid, and a great deal of carbonic acid gas, with a very small quantity of carburetted hidrogen gas.

It is probable, that the succinic acid contains a larger quantity of hidrogen, and less carbon, than the saccholactic acid. The latter does not inflame, till the moment it begins to be decomposed, while the succinic acid is very inflammable.

flammable. The succinic acid seems to approach nearer to the nature of volatile oils, while the saccholactic has more analogy to the tartarous acid and sugar.

The property observed in residuary coals by Proust, that, when treated by nitric acid, they became soluble in fixed alkalis, takes place also with the coal of saccholactic acid.

There is a difference between the coal of saccholactic acid, and that which is produced by the sublimed acid: the first yields lime on incineration; the second, potash.

XII.

Letter from Mr. Hassenfratz, *Chief Engineer and Professor of Mineralurgy at the Practical School of Mining, to Mr.* Berthollet, *on the Oxidation of Iron*[*].

Oxigen supposed to combine with iron in different proportions, but this denied by Proust.

I HAVE long been of opinion, that oxigen combines with iron in various proportions. The experiments of Proust[†], by which he shows, that only two precipitates are formed in all the solutions of iron, one of green oxide and the other of red, made me waver in my opinion, and I was on the point of giving it up; but, on comparing the quantity of oxigen mentioned by Proust as combined with iron with the quantities deduced from experiments made by other chemists, who enjoy a well deserved reputation, I found such differences in the proportions, that I resolved to suspend my judgment till fresh facts should confirm or refute this doctrine.

Different proportions assigned to the oxigen.

In fact Proust says, that the green oxide is composed of 28 parts of oxigen to 100 of iron, and the red oxide of 48 of oxigen. The experiments of Lavoisier, those which you undertook in conjunction with Messrs. Monge and Vandermonde, those of Vauquelin, and lastly those of Bucholz[‡], prove, that iron oxided at a minimum contains 30 of oxigen to 100 of iron. As to the red oxide, chemists are not

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[*] Annales de Chimie, vol. LXVII, p. 309.

[†] Memoir read to the Institute in the year 5.

[‡] Annales de Chimie, vol. LXV, p. 202; or Journal, vol. XXV, p 353.

yet

yet agreed on the proportion of oxigen it contains, some making it 42, others upwards of 50.

They may vary progressively from the least to the greatest.

Your immortal Chemical Statics appeared. The opinion you there advance, that "the proportions of oxigen may vary progressively from the term at which combination becomes possible to that at which it attains its highest degree," admits, like that of Proust, a maximum and minimum of oxidation, but it differs from him in two respects: he asserts, that these two points are the only ones, and he even fixes them; while you, in addition to the progressive variation of oxigen, leave these two extremes undetermined. Proust strenuously opposed your opinion;* and I must confess that his reasons, and the facts by which he supported them, staggered me. Thus I was on the point of giving up the oxidation of iron in various degrees, when I read Thenard's Considerations on the oxidation of metals in general, and that of iron in particular †, in which that able chemist appears to adopt a mean opinion between Proust's and yours; and where he shows, that there is a third oxide of iron, white oxide, which contains less oxigen than the green minimum oxide of the chemist of Madrid. These, with your answer to Prouſt ‡ and Darso's|| inquiry concerning the oxidation of iron, in which he says he has obtained by calcination oxide of iron containing as far as 56 of oxigen to 100 of iron, still kept my opinion in suspense.

Four oxides.

If the facts related by Thenard and Darso be true, there must be four oxides of iron: 1, white; 2, green; 3, red; 4, beyond the red as far as 56 of oxigen to 100 of iron.

Thenard.

As you have quoted the experiment of Thenard, and this chemist has the reputation of being accurate in the facts he announces, I am inclined to trust his results. As to that

Darso.

of Mr. Darso, it appears to me the more extraordinary, as all the precise experiments hitherto made had not carried the maximum oxide of iron farther than 42 or 45 of oxigen to 100 of iron, and even Proust made it no higher than 43.

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* Journal de Physique, an. 1804, tom II, p. 300.

† An. de Chim. vol LVI, p. 57: or Journal, vol XIV, p 224.

‡ Journal de Physique, an. 1805, tom. II, p. 356.

|| Journ. de Phys. an. 1806, tom II, p. 291: or Journal, vol. XVII, p. 271, 267, 328.

Doubting the possibility of fixing the limits of oxidation, and obtaining an oxide containing so large a proportion of oxigen, I imparted to you my thoughts on the subject. You hinted to me, that you had little confidence in the results announced by Mr. Darso; and, as I thought it of importance to the opinion concerning the different terms of oxidation of iron to verify the fact, I sought an opportunity of doing this.

The author's lectures on mineralurgy.

The lectures on mineralurgy, which I give every year at the school of Mining at Moustier, enabling me to make a number of chemical experiments, which are repeated by the pupils sent thither by government, who have already acquired considerable knowledge of the subject at the Polytechnic school, I determined this year to repeat the experiments of Mr. Darso, and employ Mr. Desroches, a pupil, to do the same.

Darso's experiments repeated.

Following as exactly as possible the process described by Mr. Darso*, we endeavoured to perform it with more care and attention, in order to obtain results, on which others might depend.

The iron filings first analysed.

As it was possible, that some of the substance of the test, the cupel, or the mortar, might have mixed with the iron, and contributed to the increase of its weight, we first analysed the iron filings, which we proposed to calcine. By this analysis we found, that the iron we calcined contained a little carburet of iron and silex, but in too small quantity to be weighed, with a trace of alumine and of lime.

Process described.

The iron filings, weighed separately, were put into a porcelain capsule, weighed with the capsule, and then placed under the muffle of a cupelling furnace. After having calcined them for a larger or shorter time, the capsule was withdrawn, and set to cool under a glass, that no extraneous matter might get into it; after which they were weighed with the capsule, triturated, and then weighed with the capsule afresh. These operations were repeated nine times following, and the iron increased 42·224 per cent, after which it received no further increase.

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* Certainly not at the commencement, for the filings were not stirred, nor was the air renewed, till after nine successive calcinations of several hours each. T.

I shall here subjoin a tabular view of the nine successive calcinations, that the reader may observe their progress.

Tabulated results.

	Gram.	Gain.	Loss.
The iron filings weighed 5 grammes* and the capsule	29·340		
Capsule and filings	34·340		
After a calcination of 1½ hour	34·885	0·545	
After trituration	34·885		0·000
After a 2d calcination of 2 hours	35·100	0·215	
After trituration	35·090		0·010
After a 3d calcination of 3 hours	35·530	0·440	
After trituration	35·520		0·010
After a 4th calcination of 4 hours	36·380	0·860	
After trituration	36·370		0·010
After a 5th calcination of 6 hours	36·420	0·050	
After trituration	36·405		0·015
After a 6th calcination of 4 hours	36·405	0·000	
After trituration	36·390		0·015
After a 7th calcination of 3 hours	36·390	0·000	
After trituration	36·375		0·015
After an 8th calcination of 2 hours	36·375	0·000	
After trituration	36·360		0·015
After a 9th calcination of 1 hour	36·360	0·000	

Process of the calcination irregular.

From these experiments it follows, that the increase of weight by calcination was somewhat irregular in its progress, and after the 5th calcination, which was continued for 6 hours, the oxide increased no more in weight, to whatever temperature it was exposed.

Loss by trituration.

It appears too, that in all the triturations, except the first, the oxide of iron lost weight, owing to that evaporation which is unavoidable, when we triturate a very fine substance, and this evaporation appeared to follow a law depending on the fineness of the particles of the oxide. At first, while the particles were still coarse, it was nothing; when they began to grow fine, it was 0·010; and when they were still finer, it was 0·015.

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* 77 grains English.

Calculating the increase of weight in each calcination, and the losses of each trituration, we find, that, after five successive calcinations, the iron increased 42·224 per cent: by this process therefore we cannot carry the oxide of iron farther than 42¼ of oxigen to 100 of iron.

Only 42·224 of oxigen to 100 of iron.

To satisfy ourselves whether any foreign matter beside the oxigen had combined with the iron, we analysed the oxide. This analysis, like that of the filings, yielded us an imponderable quantity of silex, and a trace of lime and alumine. Hence it follows, that no earthy substance had combined with the iron, and that the metal had lost during calcination the carbon it contained before.

No foreign substance present,

but the carbon lost.

As Mr. Darso says, that he was constantly stirring his oxide of iron during the calcination, and renewing the air in contact with it by means of a pair of bellows; and as it was possible these two causes might have contributed to the combination of a greater proportion of oxigen with the iron, we resumed the experiment where we left off, and continued the operation on 3 grammes, at 42¼ of oxigen per cent, which we had left, stirring and blowing on them. As it was possible however, if we used an iron hook to stir the oxide, that this hook might have been oxided, and part of its oxide mixed with that of the filings, so as to increase its weight, we fixed on the extremity of an iron rod a hook of glass, which we changed as often as it appeared to grow soft from the heat; and with this substance we stirred the oxided iron.

The oxide stirred and blown on during calcination

The three grammes of iron oxide were exposed seven times successively to the heat of the furnace under a muffle, and afterward triturated. The muffle was heated to the highest degree, the oxide was stirred during the calcination, and the air in contact with its surface was constantly renewed by means of a pair of bellows. In the earlier operations the oxide increased in weight; so that at the fourth there were 45 of oxigen to 100 of iron: but when it had reached this point we could not combine any more oxigen with it, so that it remained constantly at 45.

gained 2·776 more of oxigen in 4 calcinations,

but would not take up more.

These, Sir, are the results we obtained by repeating the experiments of Mr. Darso, and they confirm the opinion, that the oxidation of iron at a maximum does not exceed

Bucholz.

Gueniveau

45 of oxigen to 100 of the metal. It is true Mr. Bucholz could not combine more than 42 parts of oxigen with 100 of iron, as we did in our first experiment: but mine-engineer Gueniveau combined 44 parts of oxigen with 100 of iron in an experiment similar to that of Bucholz, as he informed me in a letter I received from him. However, as it is extremely difficult to have pure iron, exempt both from carbon and oxigen, we should consider the highest result as nearest the truth.

Darso's experiments apparently erroneous.

The white oxide still remains to be examined.

This difference in the quantity of oxigen combined with iron in the experiments of Mr. Darso and ours leads us to believe, that some substances mixed with his products, of which he did not take account. Our results bring back the question to the point at which it was before the experiments of Mr. Darso. If the precipitate obtained by Thenard be really a white oxide with less oxigen than the green, there must be three oxides of iron, the white, green, and red: to the nature and degree of oxigenation of this precipitate therefore our inquiries should be directed, to determine whether, as Proust asserts, there be but two degrees of oxigenation; one at a maximum with 45 of oxigen, and the other at a minimum with 30.

XIII.

On the muriate of Tin; by Mr. E. BERARD, *Exprofessor of Chemistry at the Medical School at Montpellier, of the Academy of Garth, &c.**

Preparation of muriate of tin in the great.

THE preparation of muriate of tin in a large way has furnished me with opportunities of observing some facts, which I think it may be of use to record. They will form a supplement to those, that have been described by various men of learning, and may serve to hasten the period, at which the art of dyeing will receive from chemistry a certain and uniform method of preparing and employing the *composition* for scarlet, which is a species of salt of tin,

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* Annales de Chimie, vol. LXVIII. p. 78.

The

Solution of tin by muriatic acid assisted by heat.

The solution of tin by muriatic acid, as directed by various authors, and as practised by Baumé, is effected by pouring on one part of this metal, in a state of extreme division, four parts of common muriatic acid, and assisting the chemical action by the heat of a sand-bath. The water serving as a vehicle to the acid is decomposed; the oxigen oxides the metal, which then combines with the acid; while the hidrogen is evolved in the state of gas, carrying with it some particles of the metal employed, which render it very fetid. But the action is slow, and the dissolution is imperfectly effected. I have observed, that a very large portion of the acid is completely lost by evaporation, and that, if you would dissolve the whole of the metal, you must not only add fresh acid, to supply the place of what is thus wasted, but keep up the action by artificial heat for several days. I tried to effect this operation in the cold, and two months were insufficient. Bayen and Charlard, in their experiments on tin, employed as much as six months.

Tin put into water, and muriatic acid gas passed into it.

Mr. Chaptal assists the chemical action between muriatic acid and tin, by placing the metal, when he prepares the acid, in the jars of Woulfe's Apparatus, in which is the water to absorb the vapours. The heat that is evolved has an excellent effect, and the action becomes very brisk toward the end of the process. But this ingenious device leaves something still to be desired, as the acid dissolves only a fourth of its weight of tin, and the solution requires to be finished by other means.

Tin exposed to nascent muriatic acid gas.

The solution of tin is still better effected by admitting into a large receiver, in which there is a sufficient quantity of the metal in a state of division, the vapours of muriatic acid evolved from a mixture of powdered muriate of soda and sulphuric acid diluted to 40° of the areometer for acids. In proceeding thus by simple distillation, the vapours of muriatic acid are pretty easily condensed and combined with the tin.

Tin in muriatic acid exposed to oximuriatic acid gas.

If the vapours of oximuriatic acid be received into a vessel containing tin and common muriatic acid, the solution is effected completely, and in a short time. The acid at 20° will then take up the third of its weight of tin.

Action of ni-

I have tried various mixtures of muriatic and nitric acid,

containing

tromuriatic acid on tin.

containing from a sixth to a tenth of the latter. They all acted on tin with extreme heat and violence, and the contents of the vessel were thrown out with violence. One part of nitric acid, or aqua fortis of the shops, at 35° of Baumé's areometer, and 12 parts of common muriatic acid at 20°, form a mixture well adapted to the solution of tin, which it effects very well, and in little time. This mixture takes up about a third its weight of tin, and the solution is carried to 45°.

Tin exposed alternately to muriatic acid and atmospheric air.

I attempted to combine the alternate action of muriatic acid and atmospheric air on tin, divided into small grains, for the purpose of dissolving it, and with complete success. With this view I filled a large wide mouthed glass bottle with finely granulated tin, covered the metal with muriatic acid at 20°, left this to act on it for a few hours, and then poured off the acid into another vessel; when it was found to have risen to 25°. The tin soon began to grow black from the contact of the atmosphere, absorbed oxigen from it, and caloric was evolved, rendering the metal very hot. A lighted candle put into the bottle was quickly extinguished. As soon as the bottle began to grow cool, I returned the acid into it, which acted with fresh force, and in a little time got to 35°. I poured it off again, to let the air act on the tin, and then returned it into the bottle afresh. Thus I continued proceeding alternately, till all action ceased. At the end of two days the solution had attained the strength of 45°. Indeed one day was sufficient for this, if a series of bottles supplied with tin were employed, so that the acid might be acting on the tin in some, while the air was acting on that in others; and by this continual action the strength might be carried even to 50°.

Solution of muriate of tin readily absorbs oxigen from the air,

The muriatic solution of tin, when fresh made, combines pretty readily with the oxigen of the atmosphere, as Pelletier, Guyton Morveau, and other celebrated chemists have observed. It is sufficient to invert a jar, filled with atmospheric acid, over a dish or widemouthed bottle filled with this solution, when the solution will continue to rise in the jar, till the whole of the oxigen is absorbed. The absorption is more rapid if the jar be filled with pure oxigen gas; and nearly the whole of the gas will be taken up in a short time. To facilitate the combination of oxigen gas with the recent solution, I caused a large quantity of atmospheric air to pass through it by means of a pair of bellows, the nozzle of which reached to the bottom of the liquid. If the solution be not fully saturated with tin, it will take up a fresh quantity in proportion as it absorbs oxigen from the atmosphere.

and oximuriatic acid gas,

Oximuriatic acid gas is eagerly absorbed by this solution, as Pelletier very justly observed. This learned chemist even proposed a solution so saturated for the purpose of dyeing scarlet; and I prevailed on several dyers to make trial of it, but none of them adopted its use. It appears, that the combination of atmospheric oxigen with it imparts to it nearly the same properties as oximuriatic acid gas. When it has absorbed a great deal of oximuriatic acid gas, it is fit for dissolving a fresh quantity of tin, and when it has dissolved more tin, its state is altered, and it is rendered again capable of absorbing oxigen gas.

which has a similar effect on it.

Crystallization of the muriatic solution of tin.

The muriatic solution of tin at 45° yields crystals of muriate of tin, by evaporation. The crystallization is effected more easily, in proportion to the length of time the solution has been kept, or to the quantity of oxigen it has absorbed. The mother water, in which the crystals are deposited, is of great density, particularly after several crystallizations, Its density is still greater, if it were evaporated before its exposure to the air: it is sometimes even slightly fuming, and will then yield crystals on being diluted with pure water. A phial that would hold 14 parts of distilled water, contained 28 of the mother water after the first crystallization: and it held 31, when the same liquid had furnished several crops of crystals by evaporation. These mother waters are capable of combining with the oxigen of the atmosphere, if the solution were not previously saturated with it: and for this purpose it is sufficient to expose them to the air, or force the air through them with a pair of bellows, as I have pointed out above for the simple solution. This combination occasions a fresh production of crystals, and if a very extensive surface of the mother water be exposed to the air, a muriate of tin crystallized in very thin and light scales will be obtained. Baumé noticed this mode of crystallization. Oximuriatic acid gas combines with the mother water with much energy; a considerable quantity of caloric is evolved, and after it is cold the liquid coagulates into a mass of silky crystals of muriate of tin. If the crystals of muriate of tin be purified by dissolution in pure water and recrystallization, they will acquire more consistence and more density.

Crystal very soluble, producing much cold.

Crystallized muriate of tin is very soluble in cold water, the solution being quickly effected, and producing a considerable diminution of the temperature. The mean diminution in my experiments was 9° of R. [20·25° F.], the temperature of the atmosphere and of the substances employed being 5° [43·25° F.]. The mixture of pure water with the mother waters produced no change of the temperature.

Mother waters distilled.

As I had observed, that these mother waters became a little fuming by evaporation, I tried the distillation of the mother waters highly concentrated, and of the crystallized muriate, to see whether I should not obtain a muriate of tin similar to that known under the name of fuming liquor of Libavius. Weak muriatic acid first came over: and then the muriate either passed into the receiver, or sublimed into the neck of the retort in a white mass, known formerly under the name of butter of tin. With the same view I passed muriatic acid gas as dry as possible through the concentrated mother water of muriate of tin when it became fuming, and yielded crystals on mixing with it pure water. But I must observe, that the fuming liquor of Libavius emits much more dense and copious vapours, that it is whiter, and that its specific gravity is greater.

Butter of tin.

Fuming liquor, weaker than that of Libavius.

The muriate has always an excess of acid, and variable,

The combinations of muriatic acid and tin in the state of solution, of crystals, or of mother water, have always an excess of acid; and from what has been said it appears, that they are all capable of infinite variations in their state. Hence we need not be surprised, if the effects they produce in dyeing are so uncertain, and so different from one another. The least variable state of muriate of tin appears to be that of crystals perfectly white and thoroughly drained. In this state this mordant ought always to be employed in dyeing, adding to it a larger or smaller proportion of nitric acid, according to the shade we wish to produce. Such a composition alone can be always uniform, and yield constant results.

except in crystals, which should always be used for dyeing.

Profiting

Preparation in the great.

Profiting by the facts detailed in this paper, it appears easy to give a simple and advantageous process for preparing the crystallized muriate of tin in the large way, yet I have met with very perplexing difficulties in the attempt to carry it into execution. These however I have at length been able to surmount, and the description of the method I have pursued will form the subject of a future paper.

XIV.

On the Formation of Acetic Ether in the Marc of Grapes: by Mr. DEROSNE[*].

Acetic ether formed in the distillation of vinegar,

and from the refuse of pressed grapes.

SOME years ago we obtained acetic ether by the simple distillation of vinegar on a pretty large scale. This year we had an opportunity of remarking its formation in the marc of grapes, from which the juice had been expressed. We were preparing some sirup of grapes, for a trial; and after the grapes had been bruised, they were subjected to the press, and the squeezed pulp was thrown into a cask. Some days after, accidentally thrusting my hand into it, I found it warm and moist; but what surprised us most was a smell of ether arising from it. Some of it was taken out, pressed, and the fluid distilled in a small alembic. The first product was in fact pure acetic ether. What came over afterward contained some likewise, but mingled with weak spirit of wine and acetous acid.

Theory of its formation.

It appears, that this marc fermented very quickly; that the acetous fermentation took place nearly at the same time with the spirituous; and that their simultaneous occurrence produced acetic ether.

Might be obtained in this way.

Thus this liquid, which, when first discovered, seemed very difficult to be obtained, appears to be formed under circumstances of no unfrequent occurrence; and perhaps, if we opportunely availed ourselves of the moment of its spontaneous formation, we might procure it in sufficient quantity, without having recourse to its artificial formation[†].

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[*] Ann. de Chim. vol. LXVIII, p. 331.

[†] In our cider countries the pomasse, or refuse of the expressed apples, might be examined for this product. C.

XV.

XV.

Late sown Clover[*].

Late sown clover.

IN 1804, the war having deranged the usual course o agriculture in Saxony, a farmer, Mr. Thaer, was unable t sow his oats and clover before July. The first mowing o the clover was a very slight crop; but the second was as fin and thick as possible. Mr. Thaer has since repeated th experiment, and he has found, that clover, when sow later than ordinary, grows more abundantly, and of bette quality; and that, provided it be sown before the middle o August, it will certainly succeed.

SCIENTIFIC NEWS.

German and French Dictionary of mining.

MR. J. B. Beurard, agent of government at the quick silver mines of the late Palatinate, has published a "German and French Dictionary, containing the Terms employed in Working Mines, in Mineralurgy, and in Mineralogy, with the Technical Terms of the Sciences and Art connected with these." It occupies one large 8vo volume and is said to have much merit.

Mode of splitting rocks by lightning.

Last summer an experiment of a new kind was tried a the village of Philipsthal in East Prussia. This was, t split a rock by means of lightning. An irod rod, similar t a conductor, was fixed in the rock, and on the occurrenc of the first thunderstorm, the lightning was conducte down the rod, and split the rock into several pieces, withou displacing it.

Plants & insects of the south of France.

Mr. Philip Salzmann, of Montpellier, has published catalogue of the plants and insects of the South of France which he proposes to collect and send half yearly to any wh may order them, at the prices annexed.

Pharmaceutical Society at Paris prize questions.

The Pharmaceutical Society of Paris has proposed th two following prize subjects for the present year.

1. To ascertain, as far as possible, whether there exis

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[*] Sonnini's Biblioth. Physico-écon. Oct. 1808, p. 223.

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in vegetables a peculiar and distinct principle, to which chemists have given the generic name of extractive.

Ought we to retain the old classification of pharmaceutic extracts; divided, according to Rouelle, into gummy, resinous, gummy-resinous, resino-gummy, and saponaceous?

Can we establish a more methodical and accurate classification by the help of chemical experiments on the principal substances, that furnish the apothecary with extracts?

To indicate, from the nature of their different constituent principles, the mode of preparation that agrees with each of them, and the nature of the menstruums that should be employed.

2. What is the present state of pharmacy in France? What part does it take in the art of healing? and of what improvements is it susceptible?

The prize for the first is a gold medal of the value of 200 francs [£8 6*s*. 8*d*.]; for the second, a gold medal of the value of 100 f. [£4 3*s*. 4*d*.]. The answers to each are to be sent, post-free, to Mr. Sureau, secretary to the society, rue Favart, No. 18, before the 1st of October next.

Medical and chemical lectures.

Dr. George Pearson, F. R. S., senior physician to St. George's Hospital, recommences his course of lectures on Physic and Chemistry, at No. 9, George street, Hanover square, on the 4th of June, at the usual morning hours; namely, the medical lectures at 8, and the chemical at 9. A chemical lecture on the cases of patients of St. George's hospital is given every Saturday morning at 9; and the practice of vaccination is carried on in Broad street, Golden square.

To Correspondents.

Mr. Verschoyle's paper, dated the 16th of April, did not come to hand till the 26th of May. I shall be happy to receive an account of his progress, and at the same time should be glad of a more precise representation and description of his apparatus. Perhaps Articles IX and X of our present number may afford him some hints on the subject of his inquiry.

METEOROLOGICAL

previously heated red hot in the fire; for these two substances gave the same results. They assert, that dry charcoal produces no water in burning, and contains no hidrogen: but the details of their processes show, that their results can be considered only as approximations sufficiently doubtful.

burned charcoal in too small quantity.

In each of their experiments these gentlemen burned only four grains of charcoal, and it is impossible to obtain accurate results with such small quantities. They estimated the density of the oxigen gas and the acid gas by weighing merely 21 cubic inches of these gasses, and determinations of this kind should be made from much larger quantities. We cannot justly give these results a preference over those which Lavoisier, Fourcroy, Vauquelin, Seguin, and Biot obtained with fifteen and even forty times the quantity, and very sensible balances; though indeed the sensibility of a balance is a very inadequate compensation for a defect of quantity in the substance weighed.

Quantity more important than accuracy of weighing.

The experiments of Messrs. Allen and Pepys do not prove, that charcoal previously heated red hot furnishes no hidrogen gas in burning: for they did not seek for this gas in the air remaining after the combustion, and it is well known, that hidrogen gas will not inflame when it exists but in very small proportion in oxigen gas.

These considerations have induced me to make known my researches on the same subjects, the diamond excepted, which I could not burn; and they have led me to some important conclusions on the most usual eudiometrical processes.

Weight of oxigen gas and carbonic acid gas.

Weights of oxigen gas and carbonic acid examined.

One of the most important points in the analysis is to determine the exact weight of oxigen gas and carbonic acid. In a process so delicate I judged it proper, not to rely on my own experiments alone, but to take the mean of the results, that appeared to me most deserving confidence. The balance I used in the following experiments was made by Fortin. When loaded with the globe employed for weighing the gasses it is sensible to a milligramme [0·01544 of a gr.].

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1810.

ARTICLE I.

Observations on the Combustion of several sorts of Charcoal, and on Hidrogen Gas: by THEODORE DE SAUSSURE. *Read at the Society of Natural Philosophy and Natural History of Geneva, August the 31st,* 1809[*].

Proportion of base in carbonic acid undetermined.

IT is well known, that the proportions of carbon and oxigen in carbonic acid gas could only be determined in a vague manner by the experiments of Lavoisier. The detail of his inquiries[†] shows that 100 parts of this gas might contain between 25 and 28 parts of carbon: and though he first adopted the latter proportion, he ultimately concluded, that the former was too great, and that the quantity of carbon contained in 100lbs. of the gas did not exceed 24lbs.[‡]

Messrs. Allen and Pepys

Among the inquiries into the subject since made, those of Messrs. Allen and Pepys[‖] are particularly to be distinguished. They found by the combustion of charcoal in oxigen gas, that 100 parts of carbonic acid gas by weight contained 28·6 parts of carbon, or of diamond, or of charcoal

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[*] Abridged from Annales de Chimie, vol. LXXI, p. 254.

[†] Mémoires de l'Acad. des Sciences, 1781.

[‡] Lavoisier's Mémoires (posthumes), vol. II.

[‖] Phil. Trans. for 1807, p. 267: or Journal, vol. XIX, p. 216.

The following are the particulars of my experiment, from which the preceding weight was deduced.

Capacity of the globe.	*Vacuum by the barometer of the air pump.*	*Temperatu of the gas.*	*Barometer corrected for temperature 12·5° or 54·5°F .*	*Weight of the gas in the globe without correction.*	*100 parts of the gas contained*
5941·6 cub. cent.	0·0056 met.	18·2°	0·7299 met.	7·52 gram.	nitrogen 2 parts.
362 cub. inches.	0·22 of an inch.	64·76 F.	28·72 in.	116·11 grs.	

In another experiment the results came out

5941·6 cub. cent.	0·005 met.	5°	0·7204 met.	7·77 gram.	nitrogen 2 parts.
362 cub. inch.	0·1967 inch.	41° F.	28·34 in.	119·97 grs.	

but as these would have given the weight of the cubic decimetre, calculated as above, 1·3445 gram. [20·7594 grs.], I thought this differed too widely from the preceding to be admitted.

Weight of a cubic decim. [60·895 cub. in.] of humid carbonic acid gas, the thermometer being at 12·5° [54·5° F.], and the barometer at 0·758 met. [29·82 in.]

According to Lavoisier	1·8470 gram.	28·5181 grs.
——— Biot	1·8591	28·7049
——— ———	1·8717	28·8995
——— my experiment	1·8565	28·6648
——— ———	1·8432	28·4594
——— ———	1·8680	28·8424
Mean	1·8576	28·6815

The following are the particulars of the experiments here calculated from.

Capacity of the globe.	*Vacuum by the barometer of the air pump.*	*Temperature of the gas.*	*Barometer, corrected for temperature 12·5 or 54·5 F.*	*Weight of the gas in the globe without correction.*	*1000 parts of the gas contained*
5941·6 cub. cent. 362 cub. inches.	0·007 met. 0·2754 inch.	14° 57·2° F.	0·7295 met. 28·7 inches.	10·476 gram. 161·752 grs.	9·29 atmosph. air.
Ditto	0·005 met. 0·1967 inch.	5·94° 42·67° F.	0·7265 met. 28·58 inches.	10·775 gram. 166·368 grs.	6·9 nitrogen, 5·8 oxigen.
Ditto	Ditto.	5° 41° F.	0·7232 met. 28·45 inches.	10·765 gram. 166·244 grs.	9·2 nitrogen 7·8 oxigen.

Direct experiments on the density of gasses are frequently omitted: but they would be of advantage, not only to correct results, that are useless from being founded on imperfect methods, but likewise to detect the errours of these methods.

Apparatus

Apparatus for the combustion of charcoal.

Apparatus for the combustion of charcoal.

The combustion was effected in the sun, by means of a lens 0.325 met. [12·8 inch.] in diameter, in a tubulated receiver with a steel cock, which contained about 2600 cent. cub. [158 cub. in.]. The charcoal was fastened by a wire of platina to a plate of the same metal, which was suspended in the middle of the receiver by a chain of platina fastened to the top of the vessel. Near its point of suspension, and in the tubulure, was fastened to the chain a small cylinder filled with muriate of lime, which had been weighed in a closed vessel previous to the experiment.

The receiver was suspended between two vertical pillars by a horizontal bar fixed to the tubulure, and crossing the pillars in the direction of their diameter. This bar could be moved up and down between the pillars, and be fixed by screws; so as to keep the receiver immersed more or less in a mercurial trough, which was deep enough to fill the receiver with mercury, when plunged into it perpendicularly with the cock open. The mercury was previously dried, and the tubulure alone was left full of common air, that the mercury might not mix with the muriate of lime. Oxigen gas from the oximuriate of potash was introduced into the receiver, by fitting to the tubulure a bladder filled with this gas, and furnished with a cock, and raising the receiver to a proper height; after which the cocks were closed.

To extract the gas from the receiver, I screwed on the tubulure a small glass globe filled with mercury. On opening the cocks, the mercury in the globe fell into the receiver, and was replaced by the gas in the latter. The pipe of the cock of the tubulure had a tube leading into the receiver to convey the mercury free of the muriate of lime.

Before I commenced the process of combustion, I always extracted in this way a part of the oxigen gas that had been introduced into the receiver, and subjected it to analysis.

Eudiometrical processes. For oxigen.

To ascertain the proportion of oxigen gas, I employed the hidrosulphuret of potash, concentrated, and impregnated with nitrogen gas; and I always compared this process with that of Volta's eudiometer. This comparison led me to several new observations on the use of this instrument.

In the trial with hidrosulphuret I followed nearly the process of Marty. I shall only observe, that I coated with suet and sand the glass stopple of the phial containing the hidrosulphuret and gas, to prevent its being completely closed. Without this precaution a vacuum is formed by the absorption of the oxigen gas, which occasions the evolution of the nitrogen that impregnates the liquid.

I let the mixture of hidrosulphuret and gas to be analysed stand at rest for five days, in which time the process is always finished. In this way I obtained more regular results, than those obtained in a few minutes by agitation according to Marty's process.

I take care, that the whole of the process is conducted at a temperature nearly uniform; for, if the hidrosulphuret be exposed to a lower degree of heat than that, at which it was impregnated with nitrogen gas before being placed in contact with the gas to be examined, it absorbs nitrogen from the latter; on the contrary, at a higher temperature it adds to it.

The eudiometrical process with the hidrosulphuret is more accurate than Volta's process, as will appear hereafter, to determine the proportion of oxigen gas when mixed with nitrogen only: but when the mixture contains carburetted or oxicarburetted hidrogen gas, it is best to employ Volta's eudiometer, or some other process in which a large quantity of water is not essential; for the liquid hidrosulphuret, or even pure water, sensibly absorbs all oxicarburetted hidrogen gasses, and makes the proportion of oxigen gas appear larger than it really is. This observation is true however only when the proportion of carburetted hidrogen exceeds one per cent of the gas analysed[*].

Absorption of inflammable gasses.

[*] The absorption of oxicarburetted hidrogen gasses by hidrosulphuret of lime has been announced by Mr. C. L. Berthollet in his excellent paper on these gasses in the Memoirs of the Society of Arcueil, vol. II, p. 79. I have observed, that the olefiant gas is absorbed in equal quantities by pure water and a solution of hidroguretted sulphuret of potash; but that pure hidrogen gas is absorbed in larger quantity by pure water than by the liquid hidrosulphuret.

To

Separation of the carbonic acid gas.

To separate the carbonic acid gas from the oxigen and nitrogen after the combustion of the charcoal, I employed the mercurial pneumatic apparatus. I immersed the mercury in the small globe, which by means of the vacuum extracted the gas from the receiver, where the combustion had been effected; and I passed the air contained in this globe into a wide tube filled with mercury, which was long enough to allow the column of gas to stand about 0·27 of a met. [10·6 inch.] high. I then introduced through the mercury one or two grammes [15 or 30 grs.] of highly concentrated solution of potash, agitated the solution in the gas occasionally, and in a few hours the whole of the acid gas was condensed. I then replaced the mercury by water and measured the absorption.

Separation of carbonic acid from oxigen by lime water not to be depended upon.

Experience has shown me, that, when the carbonic acid gas is mixed with oxigen gas nearly pure, considerable mistakes are made in separating the two gasses by lime water. This liquid, by means of the agitation required for the complete condensation of the acid gas, absorbs oxigen gas. This effect is not produced by the lime, but by the water of the solution, which is required to be in very large proportion to the volume of gas *. Liquid potash absorbs nearly the same proportion of oxigen gas as an equal bulk of lime water; but as the solution of potash may be used in an infinitely less quantity, the errour arising from it is too small to be noticed.

Potash preferable.

Lime water gives out nitrogen,

Lime water affects the result too, not merely by absorbing oxigen gas, but by replacing it by nitrogen gas, with which it is always more or less impregnated †.

but may be used when much of this is present.

When the acid gas is mingled with common air, or with oxigen gas contaminated by a large proportion of nitrogen, lime water may be employed to absorb the acid gas without any sensible errour.

Experiments on the absorp-

The following is the mean result of my experiments on this subject, under a temperature of 14° [57·2° F.]. The

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* This affects the calculation from the quantity of gas absorbed, not that from the precipitate thrown down. C.

† This would alter the state of the residual gas, it is true; but it would tend to correct the calculation made from the quantity of gas absorbed. C.

limewater I used was prepared by mixing two handfuls of quick lime with six quarts of very pure rainwater, and filtering the solution at the end of eight and forty hours. The oxigen gas was not mixed with the limewater, till it had been in contact with liquid potash. After this operation, hidrosulphuret of potash indicated in it one per cent of nitrogen.

tion of oxigen gas,

Exp. 1. A hundred parts of this oxigen were shaken 80 times, during one minute, with 400 parts of limewater by measure, in Fontana's eudiometrical tube, and were thus reduced to 96 parts. On repeating the operation, these 96 parts were reduced to 92. I made the same experiment several times with common air, but no perceptible change in its purity or quantity took place.

by limewater,

Exp. 2. A hundred parts of pure oxigen gas were mixed with 900 of limewater in a bottle closed with a glass stopple. After a minute's agitation the 100 parts were reduced to 92·5; and these 92·5 contained 8 parts of nitrogen gas. Previous to their mixture with the limewater they contained but one part, and they had been in contact with a quantity of liquid potash sufficient to absorb more than 200 parts of carbonic acid gas. This experiment was repeated by mixing with limewater 100 parts of common air, which was not perceptibly altered either in quantity or quality.

Exp. 3. A hundred parts of pure oxigen were introduced into a closed phial with 900 parts of rainwater holding in solution a fourth part of its weight of pure potash. After shaking for a minute, these were reduced to 92·5 parts. I obtained a similar result with rainwater alone.

by potash in a large quantity of water,

Exp. 4. A hundred parts of pure oxigen gas were kept for six hours in contact with four parts of concentrated solution of potash. The gas was not perceptibly altered either in quality or quantity, though the mixture was shaken several times.

& by concentrated solution of potash.

When common charcoal, or wood, or an oil, or any vegetable substance of which hidrogen constitutes a part, is burned in pure oxigen gas, hidrogen gas is always found mixed with the oxigen gas after the combustion, though the quantity of oxigen gas be much greater than is necessary

Presence of carbon in hidrogen gas hitherto considered as pure.

sary for burning the inflammable gas and all the combustible. It is obvious, that the proportion of this hidrogen gas cannot be otherwise than very small: but the electric spark will not occasion its entire disappearance. The presence of this gas can be demonstrated only by the process given by Messrs. Humboldt and Gay-Lussac for determining the combustion of a very small proportion of hidrogen in another gas. This process consists in adding 100 parts of hidrogen to 200 of the air to be analysed, and detonating the mixture with a given quantity of oxigen gas, which must be somewhat more than sufficient for burning the hidrogen. If this detonation cause a greater diminution than would result from burning the 100 parts of hidrogen added, it is to be concluded, that the gas analysed contained hidrogen gas. On this subject I shall make a few observations, to which it is of importance to attend.

The residuum of the detonation should be treated with potash, to find whether the combustion of the hidrogen gas that may be discovered furnished any carbonic acid, that no sensible quantity of carbon in the analysis may be neglected: but the estimation of the acid gas thus formed cannot be accurate, unless we deduct the carbonic acid, which the hidrogen gas used as a reagent, and deemed pure, always furnishes by its combustion, when this is effected with a surplus of oxigen gas.

Experiments on bidrogen gas obtained in various ways.

I have found to my surprise, that every kind of hidrogen gas supposed pure furnishes, when completely burned, a sensible quantity of carbonic acid gas. I have tried hidrogen gas obtained from the purest iron by means of sulphuric acid diluted with distilled water; that obtained by a similar process from zinc purified by sublimation; that from solution of tin by muriatic acid; that from the decomposition of ammonia in a redhot tube; and lastly, that from the decomposition of distilled water by Volta's pile, using platina conductors, and arranging the apparatus so that only mercury, platina, and glass were in contact with the water, and with the gasses produced. Each of these kinds of hidrogen gas, when burned with an excess of oxigen, always produced a gaseous residuum which was partly absorbed by potash, and rendered barytes water turbid by forming carbonate of barytes. The gasses were always left to stand on potash previous to the detonation: a precaution particularly necessary for the oxigen gas I employed, and which I obtained from oximuriate of potash. This gas is never free from a few thousandth parts of its bulk of carbonic acid.

Results.

A thousand parts of hidrogen gas obtained from the solution of zinc purified by sublimation, when detonated with 1000 parts of oxigen gas, afforded 3 parts of carbonic acid gas.

Hidrogen gas obtained from the zinc of the shops, which had not been sublimed, produced the same quantity of acid as the preceding.

A thousand parts of hidrogen gas from the solution of iron produced 4·5 of acid gas in a similar process.

The hidrogen gas from solution of tin in muriatic acid afforded 9 parts of carbonic acid.

That from the decomposition of water by Volta's pile, 3 parts.

That from the decomposition of ammonia, 10 parts.

To render such small quantities of carbonic acid gas sensible, I effected the absorption over mercury in a tube of such length, that the column of gas I examined was 7 dec. [27·5 inches] high, and 13 mil. [0·5 of an inch] in diameter. The process was not finished in less than 24 hours.

No nitrous vapour affected these.

It may be supposed, that the diminution of the gas by the potash was owing to the absorption of nitrous vapour, which might be formed in these combustions where a small quantity of nitrogen is always present: but I constantly made the detonation over an extensive surface of water, and left the residual gas in contact with this fluid for an hour, or a sufficient time to prevent any suspicion of the presence of this vapour.

Barytes water let fall a carbonate.

I have said, that, when barytes water was employed instead of potash, the earth was precipitated in the state of carbonate. Not to be deceived in a result so small, I introduced two ounces of an aqueous solution of barytes with excess of water into a globe of thick glass, the capacity of which was 754 cub. cent. [46 cub. inches.]

This

Experiment. This globe was furnished with a cock, and two conducting wires. After having exhausted it of air, I detonated in it a mixture of equal parts of oxigen gas and hidrogen gas obtained by dissolving sublimed zinc in sulphuric acid. After the acid gas had been absorbed by the barytes water, I extracted the residuary gas by means of the airpump, and introduced a fresh mixture of gasses similar to the former, which I detonated. These operations I repeated, till I had effected the complete combustion of 2·91 cub. dec. [177 cub. inches] of hidrogen gas at 0·73 met. [28·7 inches] of the barometer, and 18·75° of the thermometer [65·75° F.]. The carbonate of barytes formed weighed 6 cent. [0·926 of a gr.]. Admitting with Klaproth, that 100 parts of carbonate of barytes by weight contain 22 of carbonic acid, we shall find, that the 1000 parts of hidrogen gas by measure formed on combustion 2·6 of carbonic acid. This comes sufficiently near to the quantity deduced from the absorption by potash, to allow the two results to be considered as nearly similar.

Less carbonic acid obtained by the French chemists. I enter into these particulars, because the quantity of carbonic acid, which the French chemists obtained from the combustion of hidrogen gas extracted by means of zinc, in their grand experiment on the composition of water*, was equal only to a thousandth part of the hidrogen; while in my experiments the quantity of this acid was about three times as great, whether the zinc I employed were purified or not, or the hidrogen gas were obtained by any other method and considered as pure.

Water from the combustion of oxigen and hidrogen said to precipitate limewater. In the first disputes on phlogiston and the nature of hidrogen gas, some chemists announced, that the residue of the detonation of oxigen and hidrogen gasses precipitated limewater. Cavendish and Lavoisier carefully repeated this experiment, but did not observe the precipitation[*]. I have found, however, that these opposite results may be reconciled in some degree, by attending to the proportions of the oxigen and hidrogen gas. If these gasses be mixed in such proportion, that the hidrogen predominates after the combustion, scarcely any acid gas is formed, because the greater part of the carbon remains dissolved in the hidrogen gas unburned; and the latter is proportionally more oxicarburetted, than it was before the detonation. But when the combustion is effected with excess of oxigen gas, almost all the oxicarburetted hidrogen is burnt, and a sensible quantity of acid gas is obtained. Thus when I detonated 1000 parts of the purest hidrogen gas with 1000 of oxigen, I obtained at least 3 parts of carbonic acid: but I obtained only one part of this acid, when I detonated 1500 parts of the same hidrogen with 500 parts of oxigen. In the latter case the superfluous hidrogen was found to have a larger proportion of carbon in an equal volume, than before the detonation.

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* These gentlemen agree, that they could only estimate by approximation the quantity of hidrogen gas in the 987 cubic inches left after the combustion; but they estimated it at no more than a sixteen thousandth part. It appears probable to me, that it was more considerable, since phosphorus was capable of producing a detonation in it. I believe too, that this hidrogen gas was highly oxicarburetted.

This depends on the proportions of the gasses.

Pure hidrogen gas not yet known.

These results are interesting, as they prove, that we do not yet know the density of pure hidrogen gas, and that what we consider as such always contains a small proportion of carbon, and probably of oxigen; since Mr. Berthollet has found, that no pure carburetted hidrogen gas exists, those so considered always containing some oxigen[†]. It may even be questioned, whether carbon and oxigen be not essential to the constitution of the substance which we have improperly termed pure hidrogen gas. These results too indicate, that the purest distilled water contains carbon; and that the same may be said of ammonia, though the carbon is in very small proportion, and perhaps accidentally pre-

Carbon in distilled water, & in ammonia.

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[*] At that time it was not doubted, that the precipitation observed was owing to the impurity of the metals, which had been used for procuring the hidrogen Hence the existence of charcoal in zinc was admitted. Proust however could find no carbon in the black residuum left by this metal after its dissolution in acids. To me it appears very probable, that the carbon of the hidrogen gas evolved by zinc, as well as by several other metals, is to be ascribed in part to the water decomposed.

[†] Mém. de la Société d'Arcueil, tom. II.

sent in these two substances. It may act an important part however in vegetation, and in a great number of other processes.

On the condensation of nitrogen in Volta's eudiometer.

When hidrogen gas is burned slowly, rapidly, or instantaneously in a mixture of oxigen and nitrogen, the last mentioned gas is in part condensed; combining, according to the proportions of the mixture, either with the oxigen alone, or with the oxigen and hidrogen. These effects are sufficiently great in some circumstances, to occasion considerable errours in eudiometrical experiments by Volta's process. This will appear from the following instances, which have been repeated a great many times.

Two hundred parts of hidrogen gas procured by means of zinc, and 200 parts of oxigen, in which the hidrosulphuret of potash indicated 5·75 of nitrogen, left, after their detonation and the action of potash, a residuum equal to 102·5 parts. This result indicates at least 1·7 of nitrogen in the 200 of hidrogen*.

Two hundred parts of the same oxigen gas, and 200 of the same hidrogen, were burned with 200 parts of nitrogen extracted from common air. The residuum of the detonation, after the action of the potash, was only 197·5 parts; but according to the preceding experiment it ought to have been 202·5, if the nitrogen added had not sensibly affected the result.

To judge of the quantity of nitrogen condensed, I treated these 197·5 parts with potash, and with hidrosulphuret, by which they were reduced to 105·45. Now the quantity of nitrogen introduced in this experiment was 100 + 5·75 + 1·7 = 107·45. The combustion therefore condensed 107·45 — 105·45 = 2 parts of nitrogen. This result was the mean of a great number of observations.

In the preceding experiment, where 200 parts of hidrogen, containing 1·7 of nitrogen, were burned with 200 of oxigen, in which 5·75 of nitrogen were included, the latter was condensed, but much less than in the present. For the 102·5 parts of gas remaining after the detonation were reduced by potash and hidrosulphuret to 6·84 parts, which indicate a condensation of 0·6 of nitrogen.

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* I admit, with Gay-Lussac, that 200 parts of hidrogen condense 100 of oxigen. But I must observe, that the formation of carbonic acid gas, which accompanies the combustion of the purest hidrogen, may modify this assertion in a very slight degree, or prevent it from being proved to less than a thousandth part of the volume of the hidrogen.

Hence it follows, that the errours of Volta's eudiometer are not equal with all mixtures, and that the indications of this instrument are more accurate, in proportion as the gas analysed contains less nitrogen.

With atmospheric air the errours balance each other.

When atmospheric air is analysed by Volta's eudiometer, adding to it an equal bulk of hidrogen according to the usual practice, the mixture, when detonated, experiences about the same degree of condensation, as if the process with hidrosulphuret had been employed. I satisfied myself however by means of nitrous gas, that all the oxigen gas of the atmospheric air had not been destroyed in the combustion: but as some nitrogen was condensed, this compensated for the oxigen not destroyed. In this case the indication of Volta's eudiometer was accurate only in appearance.

Compounds formed in the combustion of hidrogen and oxigen contaminated with nitrogen.

Lavoisier, Fourcroy, Seguin, Vauquelin, and Cavendish, found, that water formed by the combustion of hidrogen and oxigen mingled with nitrogen sometimes contained nitric or nitrous acid, and in other cases appeared perfectly pure. The experiments of the French chemists indicate, that a slow combustion is the only mean of preventing the developement of the acid. Cavendish however had said, that when the combination of the gasses was effected by detonation, or a rapid combustion, pure water was obtained free from acidity, when the gas, according to his expression, was phlogisticated after the combustion: and, on the contrary, that nitric acid was formed, when the oxigen gas predominated in the combination. I have found, that the observation of Cavendish was accurate in this respect, that the water is acid, when there is an excess of oxigen gas; but that it does not redden blue colours, when there is an excess of hidrogen after the rapid combustion. At the same time however I have observed, that the purity of the water is apparent only, and that the want of acidity is not owing to the absence of nitric acid, but to its neutralization by ammonia, which is always formed with the nitric acid, either by ra-

Nitric acid always,

but in certain cases neutralized by ammonia.

pid or slow combustion, when the hidrogen gas predominates. Into a globe previously exhausted of air I introduced a mixture of two parts of pure hidrogen gas and one of oxigen contaminated with five per cent of nitrogen. I detonated these by means of the electric spark; and then supplied the place of the gasses destroyed by a similar mixture. Having repeated these operations several times, till the gasses would no longer inflame, in consequence of the superabundance of hidrogen and nitrogen, which gradually accumulated in the globe, I obtained 3·25 gram. [50·2 grs.] of water free from acidity. But this water left by spontaneous evaporation about 13 thousandths of a gramme [0·2 of a gr.] of crystallized nitrate of ammonia; which, being triturated with potash, gave out an evident smell of ammonia, and formed very decided nitrate of potash.

Nitrate of ammonia obtained from various inflammable gasses.

I burned very slowly in atmospheric air a stream of hidrogen gas issuing from a gazometer under the mouth of a glass jar, and collected the water that fell from it after condensation on the sides of the vessel. This yielded nitrate of ammonia by spontaneous evaporation, but in less proportion than in the preceding process, which was conducted in a close vessel. In like manner I obtained the same salt by combustion of all the oxicarburetted hidrogen gasses I tried. The formation of ammonia in these combustions is a fact not hitherto observed.

(To be concluded in our next.)

II.

Description of a Clock Escapement: by Mr. GEORGE PRIOR, *jun. of Otley, in Yorkshire.**

SIR,

Escapement invented,

SINCE I had the pleasure of seeing you, when my father and I were in London, I have invented and made the machine now sent to your address; and I will thank you to lay it before the Society of Arts &c. the first opportunity.

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* Trans. of Soc. of Arts, vol. XXVII, p. 200. The silver medal and twenty five guineas were voted to Mr. Prior for this invention.

will answer for any pendulum.

This escapement will do for a pendulum of any length, and the friction is so small, that it does not require any oil, for it may be made as little as the tooth of the wheel can sufficiently touch the impelling spring, and yet be properly scaped.

Requisites.

It is necessary, that the detent spring, the impelling spring, and the pendulum should all spring from one right line or centre; and that the impelling spring should be so much stronger than the detent spring, as will always be sufficient to unlock the wheel.

Its mode of action.

The machine being wound up, and the pendulum put in motion toward the left side, the impelling spring unlocks the wheel, when a tooth falls against the pallet, where it remains until the pendulum returns; then moving the impelling spring, and the wheel being free, the weight on the axis causes it to advance while it escapes one tooth off the end of the pallet or spring, and another tooth is locked against the detent spring, as before, while the pendulum returns, and the impelling spring again unlocks the detent; as the impelling spring moves towards the left, the resistance it meets with by the elasticity of the detent spring in unlocking is returned to it, so that there is no more power lost than what was caused by the pressure of the wheel against the detent spring; by which means it is freed from the loss of the maintaining power, which is evident in all detached escapements I have seen, by the pendulum or crutch, &c., touching a spring or lifting a lever to regain the position for unlocking the wheel.

I am, Sir,
Your most obedient humble servant,
GEORGE PRIOR, Jun.

P. S. The screw in the pendulum rod is to adjust the machine when fixed up.

Description of Mr. Prior's Escapement. Plate V, *Fig.* 1, 2.

Description of the escapement.

Fig. 1 is a side, and fig. 2 a back view of it, which is supposed to be taken from behind the clock; *a* represents the axis of the swing wheel, or last wheel of the train of the clock: *b d* is the swing wheel fixed upon it, having 30 serrated teeth, it is turned round in the direction from *b* to *d*, by the maintaing power of the clock, (in the model this power is supplied by the descent of a small weight attached to the end of the small line, which is coiled round the barrel *f*, on the axis of the swing wheel); *g* is a spring detent, which locks against one of the teeth of the swing wheel, and this prevents its running down, by the action of the maintaining power; *h* is another spring detent, which is called the impelling spring, when left at liberty, it unlocks the former by pushing against the end of the small arch, fig. 2, *e*, fastened to the detent *g*, and thus removing the end of the detent which obstructed the wheel's motion; *l* is the rod of the pendulum suspended by a cock screwed to the back plate of the clock; a small piece of brass *k*, fig. 1, projects at right angles from the impelling spring *h*, so as to intercept the pendulum rod in its vibration, and at this place a small screw is put through the pendulum rod *l*, the point of which moves the impelling spring back; a small pin is fixed to the frame in a line between the point of suspension of the pendulum, and in the centre of the swing wheel, against which the impelling spring stops when at liberty.

The action. Supposing the pendulum to be vibrating backwards and forwards, and the wheel locked as in the figure, the pendulum swinging from *m* to *n*, fig. 2, the impelling spring *h* follows by its elasticity, until the pendulum *l* arrives at the perpendicular: at this period the impelling spring comes to rest against the end of the arc *e*, which it pushes back, so as to release the tooth of the wheel from the detent spring *g*; the wheel now moves round a very small space before it meets the end of the impelling spring *h*, and is stopped thereby, in the mean time the pendulum continues its motion the extent of its vibration towards *n*, when it returns, and arriving at the perpendicular, it meets the impelling spring *h*, and carries it along with it, until the tooth of the wheel which rests against it, escapes from the end of it, and another tooth of the wheel comes to rest against the spring detent *g*. The succeeding vibration of the pendulum repeats the same operation.

III.

An Eye Bath, to clear the Eye from extraneous Matters, and to assist the Sight: by Mr. John Duckett Ross, *No. 55, Princes Street, Leicester square.*[*]

SIR.

BEING employed in the jewellery business, I have frequently suffered greatly from extraneous substances getting into my eyes, at different times, while I have been at work; and I have witnessed many accidents of a similar kind, which have happened to enamel-grinders, turners in metal, jewellers, lapidaries,, and other artificers. These circumstances led me to attempt some means, which would relieve such misfortunes, and also strengthen my own eyes, which were naturally weak. I have now succeeded in inventing an eye bath, possessing all these advantages; which I beg leave to lay before the Society of Arts &c., and hope it will be patronized by them.

Eye liable to accidents in certain employments.

Eye bath.

The machine I have sent forms an elegant ornament for a lady's or gentleman's dressing-room, and has been found very serviceable in use, of which I will furnish the Society with certificates.

I remain, Sir,
Your most humble and obedient servant,
JOHN DUCKETT ROSS.

SIR,

I thought it might not be improper to acquaint you, that Mr. Carpue has, according to a promise he made me, this day honoured my invention, by expressing his fullest satisfaction of it before the Medical Board; and that surgeon-general Keate, and Mr. Gillham ,late chief surgeon to the Coldstream

Recommendations of it.

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[*] Trans. of the Soc. of Arts, vol. xxvii, p. 203. Fifteen guineas were voted to Mr Ross for this invention.

Coldstream regiment, have, in consequence, favoured me with their orders: and I am farther assured, that there is great hopes of my invention being generally adopted for the use of the army and navy.

I am, Sir,
Your most obedient humble servant,
JOHN DUCKETT ROSS.

Certificates were received from several other persons, stating, that they considered Mr. Ross's invention for the eyes calculated to produce many excellent advantages to the public, and likely to become extensively useful.

Description of Mr. Ross's Eye Bath. Plate V, Fig. 3, 4, 5, and 6.

The eye bath described.

Fig. 4, Plate V, is a perspective view of the eye bath, which is preserved in the Society's Repository. This apparatus is supported on a pedestal or tripod. The bath part is represented on a larger scale in section, fig. 5, where *a b* represents a glass vessel, which has a neck at the lower end, and an aperture at its vertex, as is shown in the plan, fig. 6, to fit the eye. The neck is cemented into a brass tube *c c*, which is supported, by being screwed into an ornamental piece of brass work at the top of the pedestal. This tube encloses a common pewter syringe, the end of which is cemented into the neck of the glass vessel, as the section sufficiently explains. The handle *e* of the syringe has a piece of brass screwed to it, which slides up and down, between two pieces of brass at *h*, in the pedestal, and a glass dish *i* is fixed below the frame, to receive any water which may be spilled by accident. When the instrument is used, the glass vessel is to be partly filled with water, (or any other liquor with which the eye is to be syringed,) so as to cover the orifice of the syringe; the patient then places his eye over the aperture in the glass vessel *a b*, and suddenly lifts up the brass slider at *h*, to which the handle of the syringe is fixed, so as to force the liquor contained in the syringe through that in the glass vessel into the eye; the liquor which covers the point of the syringe takes off the force with which the liquor would be thrown into the eye, so as to render the operation not in the least painful.

Its operation gives no pain.

A more commodious, though less elegant form of the same apparatus is shown in fig. 3. The syringe *a* is here placed horizontally, its point being turned up. It is soldered into a vessel of japanned tin, and the glass *e*, which is here globular, is laid loose on the japanned vessel *d*, which contains the liquor that surrounds the point of the syringe. This apparatus, fig. 3, is intended to be placed on a table when used.

A more commodious but less elegant form.

IV.

On Telegraphic Communications, in a Letter from RICHARD LOVELL EDGEWORTH, *Esq. M. R. I. A.*

To Mr. NICHOLSON.

SIR,

IN the second volume of your quarto Journal, you have given a summary of an essay on telegraphs, which I had published in the Transactions of the Royal Irish Academy. The essay concluded in these words, page 324:—" The thing itself must sooner or later prevail, for utility convinces and governs mankind; and however inattention or timidity may for a time impede its progress, I will venture to predict, that it will at some future period be generally practised, not only in these islands, but that it will become a means of communication between the most distant parts of the world, wherever arts and sciences have civilized mankind."

Essay on telegraphs in the Irish Trans.

Telegraphic communications must become prevalent.

Since that time I have seen various inventions similar to each other for the purposes of telegraphic communication, and in particular I have noticed one in your last number, called a homograph. Now I actually practised such a contrivance twelve or thirteen years ago, and I had during last month drawn up a detailed view of the scheme for the purpose of recommending it, not for the navy, but for the army. I do not, however, by any means, wish to derogate from the merit or the claims of the gallant officer, who serves his country with so much energy both of body and mind, but to

Various inventions for the purpose.

to give him the advantage of what much experience has taught me. Lieut. Spratt points out the convenience of having some dark object behind the man who makes his telegraphic signals. This observation shows me, that he has limited his project to the communication of intelligence to a single post from the place whence it is dispatched. Now there are no bounds, except the ocean, to the distances between which intelligence may be conveyed by men alone, without the intervention of any apparatus but a telescope.

Communications need not be limited to a single person,

but may be conveyed over any extent of land.

For this purpose the signalman must be *legible* both behind and before.

Repetition best shows that the signal is understood.

In Mr. Spratt's arrangement there is a signal to denote, that the operator is understood; the best means of ascertaining this, is a repetition of the signal by the person that receives it: and this, through a long line of communication, gives no delay except at the second station.

I shall not encumber your Journal with any particular detail of the arrangements, which I had made for this scheme; they may be varied ad infinitum.

Nobody but a fastidious critic will find fault with the gallant lieutenant for calling his telegraph a homograph; it should however be called an andrograph, or homoscribe, or by some English name.

Generals should not neglect the resources of art.

I take this opportunity of observing, that if generals of large armies would employ the resources of art as well as those of mere physical force, they would save much time and blood; they would avoid much disappointment and disgrace; and whether they ultimately failed or succeeded, they would have the satisfaction of knowing, that they had neglected no reasonable means of ensuring success.

I am, Sir,

Your obedient servant,

RICHARD LOVELL EDGEWORTH.

New

V.

New Theory of the Diurnal Motion of the Earth round its Axis. In a Letter from Professor WOOD.

To Mr. NICHOLSON.

Richmond Academy, State of Virginia,
SIR, *4th Feb.* 1810.

THE theory, of which this circular letter gives you a very brief account, is published in English; but being desirous of having the opinion of several of the mathematicians on the continent of Europe, as to the correctness of the principle, I was induced to write the circular in the French language. If it appear to you to merit notice, some account of it in your Journal will much oblige,

Sir,
Your obedient servant,
JOHN WOOD.

SIR,

New theory of the rotary motion of the Earth.

I have just published in this city a work entitled, "A new Theory of the Diurnal Rotation of the Earth, demonstrated upon mathematical Principles, from the Properties of the Cycloid and Epicycloid: with an Application of this Theory to the Explanation of the various Phenomena of the Winds, Tides, and those stony and metallic Concretions, which have fallen from Heaven upon the Surface of the Earth."

Proposed to mathematicians in general.

This theory I have made a point of communicating to all those whom I consider eminently distinguished for their knowledge of the mathematics. It is for them to be my judges, and to decide on the solidity of the principle, which forms the basis of my work. In short, I wish to know their opinions for or against me. Allow me therefore, sir, to impart to you as concisely as possible the circumstance, which gave rise to my theory, and to the fundamental principles it includes.

Bet on the motion of a wheel.

In the beginning of last summer two gentlemen in Richmond laid a considerable wager on the following question: "Do the top and bottom of a cart or carriage wheel, when in motion, move with equal or unequal velocities?"

The top has greater velocity than the bottom.

When the question was first proposed to me, I certainly was of opinion, that there could be no difference in the velocity of any point in the same circumference of the wheel: but upon reflecting, that every point of a carriage wheel moving along a right line in a horizontal plane describes a cycloid, a leading property of which curve is for the generating point to describe unequal arcs in equal times, I was convinced of my errour; and perceived, that any point in the upper semicircle of the wheel must move with greater velocity than the corresponding and opposite point in the under semicircle.

This applicable to the motion of the Earth.

This truth immediately suggested the application of the same principle to the motion of the Earth; for it is evident, that the motion of any point on the Earth's surface, with the exception of the two poles, being compounded of two motions, a rotary motion round the axis of the Earth, and a progressive motion along the plane of the ecliptic, will also describe a curve of the cycloidal, or rather epicycloidal species, possessing a similar property with the common cycloid generated by a carriage wheel.

Important consequences deducible from this,

as the centrifugal force must vary.

This accounts for the tides, trade-winds, &c.

The cycloidal motion on the points of the Earth's surface being once established, several important consequences obviously present themselves. For it is manifest, if every point in the same parallel of latitude vary its velocity in revolving round the axis of the Earth, the centrifugal force of that point must also vary; that is, when the velocity of the point is greatest, the centrifugal force will also be greatest; and on the contrary, when the velocity of the point is least, the centrifugal force will also be least. This variation in the centrifugal force of every point on the Earth's surface, during a diurnal rotation, necessarily affecting the fluids which encompass the Earth; it appeared to me, that the phenomena of the tides, the trade winds, and several other phenomena in nature, might thereby be explained. On this subject I published, in the month of May, several essays in the Inquirer and Virginia Argus of Richmond. These essays having received the approbation of many gentlemen well acquainted with the mathematical and physical sciences, I was induced to enter into a farther investigation of the effects arising from this principle of cycloidal motion. The result of this investigation is contained in the work abovementioned: and to give you a general idea of the principles on which my theory is founded, I shall recite those propositions of my work, which appear to me most important.

Prop. 8, Book I. If the semicycloid A a *A* be described **Prop. 1.**
by the point A of the circle A D B E, Pl. V, fig. 7, revolving from B to A; and the semicycloid B b *B* be at the same time described by the opposite point of the generating circle; then the cycloidal arcs A a and B b, described in equal times, will be to each other as the chord A F of the circular arc A F to the difference between the diameter A B of the generating circle and the chord *B* H of the supplement of the same arc.

Prop. 9. The velocity of the point A at a is to the velo- **Prop. 2.**
city of B at b, as $\sqrt{BL}$ to $\sqrt{AL}$.

Prop. 12. If the circle A D B E move from B to *A* with **Prop. 3.**
two uniform motions, a rotatory motion and a progressive motion, so that the two opposite points A and B describe two semicycloidal curves A a *A* and B b *B*, and the progressive velocity be to the rotary velocity as n to 1: then the velocity of the point A in any place, a, will be to the velocity of the point B in any place, b, as

$$\sqrt{1+\frac{\overline{n \times AC + AC - AL}^2}{2\,AC \times AL - AL^2}}$$

$$\text{is to } \sqrt{1+\frac{\overline{n \times AC + AC - BL}^2}{2\,AC + BL - BL^2}}$$

In order to apply this formula to the motion of the Earth, I suppose it to move along the chord of an elliptical arc every twenty-four hours, instead of the arc itself, over which it really moves: and having shown, that the velocity of any part on the surface of the globe, except at the poles, is greatest at noon, and least at midnight, I prove, that the velocities of any two opposite points of the equator are to each other, as

$$\sqrt{1+\frac{64{\cdot}4-AL^2}{2AL-AL^2}} \text{ is to } \sqrt{1+\frac{64{\cdot}4-BM^2}{2BM-BM^2}}$$

Difference of velocities at the equator.

From this formula I have deduced, that the velocity of any point of the equator at noon is to the velocity of the same point at midnight, as 3690 to 3502, or as 1·053 to 1 *. In like manner I have found, that the velocity at 1 o'clock P. M. is to the velocity at 1 A. M. as 248·9 to 241·4, or as 1·032 to 1.

Effects on gravitation,

and consequently on the sea, &c.

I afterward calculate the effects, which this difference in the velocities of two opposite points in the same parallel of latitude would produce on the force of gravity at the surface of the Earth, and I find, that, under the equator, bodies lose at noon a 9375th of their weight. I then demonstrate, that the effect, which this difference in the gravity of bodies produces upon the matter and fluids on the surface of the globe is 306 times greater than † the effect produced by the attraction of the moon, and 1372 times greater than any effect produced by the sun.

Application to phenomena.

This principle I have employed to explain the tides, the trade winds, and the phenomena of falling stones ‡. My theory necessarily leads into researches of too great length, to find a place in this brief analysis: I confine myself therefore, Sir, to request your serious examination of the principle of cycloidal motion, which I ascribe to every point of the surface of the globe, and the effects which this motion must produce on bodies at its surface. Your ideas on this subject will highly oblige me.

I have the honour to be,
Sir,
Your very obedient, and very humble servant,
JOHN WOOD.

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* In the work itself, which is now before me, there is a list of errata, which makes these numbers as 3718 to 3602, or as 1·034 to 1. C.

† It should be "306 times as great as." C.

Meteoric stones.

‡ Prof. Wood supposes the stones, that fall from the atmosphere, to be projected into it from volcanoes: and that, as the point from which they are thrown has its rotary velocity increased or diminished, while the stones retain that impressed on them at the time of their projection, they must consequently reach the Earth at a greater or less distance east or west of the volcano. C.

Method

VI.

Method of securing the Beams of Ships, without wooden Knees made of one Piece: by Mr. GEORGE WILLIAMS, *Master Carver at his Majesty's Dock Yard, Chatham*[*].

SIR,

Advantages of this method of securing a ship's beams.

I Submit to you, for the inspection of the Society, the following particulars of my invention for the better securing of the beams of ships of war, East and West India ships, and all others where strength, dispatch, room, and cheapness are required. In this method less iron in weight, and fewer bolts are necessary, than in the iron knees before in use; there is also less strain upon the bolts, as the block underneath is morticed both into the beam and side of the ship, as well as bolted.

Upon this plan the work is all done under the hand, which is executed much quicker than in the former plan, where the work is all done over hand, and where great nicety is requisite in making the bolt-holes which pass through both the iron stays. In my method much more room is also gained between decks for stowage and working the guns, and even a porthole may be made under the beam itself.

Saving.

I calculate the saving in a 74-gun ship or East Indiaman to be as follows, viz.

	Ton.	*Cwt.*	*qrs.*	*lb.*		*s.*	*d.*		*£.*	*s.*	*d.*
Copper bolts,	2	1	3	9	at	1	4	*per lb.* ··	305	8	0
Iron,	2	12				56		*per cwt.* ··	145	12	0
Three men and one boy's time for a month ····									45	17	6
Timber ································									50	0	0
									£546	17	6

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[*] Trans. of the Society of Arts, vol. XXVII, p. 142. The silver medal was voted to Mr. Williams for this invention.

The models I have sent will, I trust, clearly explain to the Committee every circumstance, which will be thought necessary.

I am, Sir,
Your respectful humble servant,
GEORGE WILLIAMS.

Description of Mr. Williams's Method of connecting the Beams of Ship's Decks to their Sides, Plate VI, Fig. 1, 2, and 3.

Explanation of the plate.

Fig. 1 is a horizontal plan of a portion of a ship's side; the planks of the deck being removed, to show the ends of two of the beams A A, which extend across the vessel. B B is the outside planking of the ship; C C the sections of the timbers or ribs; and to these the beams A A are fastened by beaten iron triangular braces *a a*, similar in form to the Roman capital letter A. These are let into the beams, and attached there at the angle by three bolts going through them. Fig. 2 is a front view of only one beam, where the spectator is supposed to be looking towards the ship's side; and fig. 3 is part of a cross section of the vessel's side; the same letters are used as in the other figures. By inspecting these, it will be seen, that the ends of the brace *a a* are turned up and bolted to the timbers of the ship's side by two bolts passing through each end, and through the timbers and the outside planks; by which means the beams are secured from lateral motion: and to brace them in a vertical direction the wooden block H, fig. 3, is fitted in beneath them, and two iron straps bolted on them; one end of each of these straps is attached to the deck beams by the same bolts as the upper brace *a a*; the other ends are bolted against the inside planking, and an oblique bolt *h*, fig. 3, passes through the middle of each strap and the ship's side; *l l l*, fig. 1, are the small intermediate beams, answering to the joists of a floor, to which the planks of the deck are spiked down; *m*, fig. 1, represents one of the planks, and the dotted lines show the joints of the others. In fig. 3, these planks are shown, and the other beams to make all sound and firm, which were removed in the other figure to show the braces.

VII.

VII.

Method to prevent the Accidents which frequently happen from the Linchpins of Carriages breaking or coming out: by Mr. J. Varty, of Liverpool, Coachmaker[*].

SIR,

HEREWITH you will receive a model of an axle-tree for public machines, intended to prevent the wheel from coming off, if the linchpin should break, and thereby prevent many dangerous consequences. When the idea first suggested itself to me, I put it in practice in a stage coach, which has since run from Liverpool to Litchfield, a distance of eighty-four miles, six days per week, for the last six months. During that time several instances have occurred in which the linchpins have broke or come out, but owing to this contrivance no accident has happened therefrom. We almost daily hear of stage coaches being upset, which more frequently arises from linchpins breaking than from any other cause.

Contrivance to prevent accidents from the linchpin of a wheel breaking or slipping out.

In offering this model to the Society of Arts &c. for their inspection, I anticipate the pleasure of their sanction, as I can furnish satisfactory vouchers of its proved utility.

I am, Sir,
Yours, respectfully,
J. VARTY.

Description of Mr. Varty's Linchpin.

Mr. Varty's contrivance is shown in fig. 4, and 5, Plate VI. Fig. 4 is a section of the nave of a carriage wheel, with the axle-tree A A in it; and fig. 5 is a separate view of the axletree. *a*, fig. 5, is the linchpin detached; it is put through an oblong hole in the axle as usual, but there is likewise an additional linchpin *b*, to make it complete, which is fixed in a recess cut for it in the axle, and turns on a pin (as is shown in the figure) into the hole left by removing

Described.

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[*] Trans. of the Society of Arts, vol. XXVII. p. 145. For this invention the silver medal was voted to Mr. Varty.

moving the linchpin *a*, when the wheel is to be taken off; but if the linchpin *a* should accidentally get out, this additional pin *b* would effectually keep the wheel on, as its hanging position does not at all tend to shut the pin up into the axle, but the contrary. The common linchpin *a* is put in downwards, and its weight may also tend to keep it in, and is secured in the usual way by a strap, the holes for which may be seen in the figures. The whole, when in its place, is shown at fig. 4.

VIII.

An Analysis of several Varieties of British and Foreign Salt (Muriate of Soda), with a view to explain their Fitness for different economical Purposes. By WILLIAM HENRY, *M. D. F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester*[*].

SECT. I. *General Observations.*

Object of the author.

IN undertaking the series of experiments, described in the following pages, I had not so much in view the discovery of novelties in science, as the determination, by the careful employment of known processes, and by the improvement of methods of analysis, of a number of facts, the establishment of which (it appeared to me probable) might have an influence on an important branch of national revenue and industry.

British salt generally considered of inferior quality.

An opinion has for some time past existed, and I believe has been pretty general both in this and other countries, to the disadvantage of British salt as a preserver of animal food; and a decided preference has been given to the salt procured from France, Spain, Portugal, and other warm climates, where it is prepared by the spontaneous evaporation of sea water. In conformity with this opinion, large sums of money are annually paid to foreign nations, for the supply of an article, which Great Britain possesses, beyond almost any other country in Europe, the means of drawing from her own internal resources. It becomes, therefore, of much consequence to ascertain, whether this preference of foreign salt be founded on accurate experience, or be merely a matter of prejudice; and, in the former case, whether any chemical difference can be discovered, that may explain the superiority of the one to the other.

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[*] Philosophical Transactions for 1810, Part I.

This of importance to be ascertained.

The comparative fitness of these varieties of salt for the curing of provision, which has been a subject of much controversy among the parties who are interested, can be decided, it is obvious, in no other way, than by a careful examination of the evidence on both sides. Where evidence, however, is doubtful, and where there exists, as in this case, much contrariety of testimony, it cannot be unfair to yield our belief to that, which best accords with the chemical and physical qualities of the substances in question. Again, if salt of British production should be proved to be really inferior in chemical purity to foreign salt, it would be important to ascertain, as the basis of all attempts toward its improvement, in what, precisely, this inferiority consists. It seemed desirable, also, to examine whether any differences of chemical composition exist among the several varieties of home-made salt, which can explain their variable fitness for economical purposes.

The present examination impartial.

Such were the considerations that, induced me to undertake an inquiry, which has occupied, for several months past, a large share of my leisure and attention. I began the investigation, wholly uninfluenced by any preconceived opinions on the subject; and I had no motive to see the facts in any other than their true light, since I have no personal interest, either directly or remotely, in the decision of the question.

Sources of British salt.

The principal sources of the salt, which is manufactured in this country, are rock salt, brine springs, and seawater. The first material is confined entirely, and the second chiefly, though not wholly, to a particular district of Cheshire. Of the extent and boundaries of this district, the process of manufacture, and other circumstances interesting to the mineralogist as well as to the chemist, an ample and excellent history has been given by Mr. Henry Holland, in the

Agricultural Report of the county of Chester*. From his account I shall extract, in order to render some parts of this memoir more intelligible, a very brief statement of the characteristic differences of the several varieties of salt, which are prepared in Northwich, and its neighbourhood.

Preparation of stoved or lump salt.

In making the *stoved* or *lump salt*, the brine is brought to a boiling heat, which, in brine fully saturated, is 226° of Fahrenheit. This temperature is continued during the whole process; and as the evaporation proceeds, small flakey crystals continue to form themselves, and to fall to the bottom of the boiler. At the end of from eight to twelve hours, the greatest part of the water of solution is found to be evaporated; so much only being left, as barely to cover the salt and the bottom of the pan. The salt is then removed into conical wicker baskets, termed *barrows*; and, after being well drained, is dried in stoves, where it sustains a loss of about one seventh of its weight.

Separation of its impurities.

On the first application of heat to the brine, a quantity of carbonate of lime, and sometimes a little oxide of iron, both of which had been held in solution by an excess of carbonic acid, are separated; and are either removed by skimming, or are allowed to subside to the bottom of the pan, along with the salt first formed, and with some sulphate of lime; and are afterward raked out. These two operations are called *clearing* the pan. Some brines scarcely require them at all, and others only occasionally. The whole of the impurities, however, are not thus removed; for a part, subsiding to the bottom, forms a solid incrustation, termed by the workmen *pan-scale*. The portion of this, which is lowest, acquires so much induration and adhesion to the pan, that it is necessary to remove it, once every three or four weeks, by heavy blows with a pick-axe. These sediments are formed, also, in making the other varieties of salt.

Common salt.

In preparing *common salt*, the brine is first raised to a boiling heat, with the double view of bringing it as quickly as possible to the point of saturation, and of clearing it from its earthy contents. The fires are then slackened, and the evaporation is carried on for 24 hours, with the brine heated to 160° or 170° Fahrenheit. The salt, thus formed, is in quadrangular pyramids or hoppers, which are close and hard in their texture. The remainder of the process is similar to that of making stoved salt, except that, after being drained, it is carried immediately to the store house, and not afterward exposed to heat, an operation confined to the stoved salt.

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* Published in 1808.

Large grained flakey salt.

The *large grained flakey salt* is made with an evaporation conducted at the heat of 130 or 140 degrees. The salt thus formed is somewhat harder than common salt, and approaches more nearly to the cubic shape of the crystals of muriate of soda.

Large grained or fishery salt.

Large grained or *fishery salt*, is prepared from brine heated only to 100° or 110° Fahrenheit. No perceptible agitation, therefore, is produced in the brine, and the slowness of the process, which lasts from 7 or 8 to 10 days, allows the muriate of soda to form in large, and nearly cubical crystals, seldom however quite perfect in their shape*.

Uses of the different kinds.

For ordinary domestic uses, stoved salt is perfectly sufficient. Common salt is adapted to the *striking* and salting of provision, which is not intended for sea voyages or warm climates. For the latter purposes, the large grained or fishery salt is peculiarly fitted.

Scotch salt from seawater.

On the eastern and western coasts of Scotland, and especially on the shores of the Frith of Forth, large quantities of salt are made by the evaporation of seawater. In consequence of the cheapness of fuel, the process is carried on, from first to last, by artificial heat, at a temperature, I believe, equal or nearly so to the boiling point, and varying, therefore, according to the concentration of the brine. The kind of salt, chiefly formed in Scotland, approaches most nearly to the character of stoved salt. In some places a salt is prepared, termed *Sunday salt*; so called, in consequence of the fires being slackened between Saturday and Monday, which increases considerably the size of the crystals.

Sunday salt.

Dr. Thomson.

I am indebted to Dr. Thomson of Edinburgh (who gave me his assistance with great zeal and alacrity) for an opportunity of examining upwards of twenty specimens of Scotch salt, prepared by different manufacturers. That distinguished chemist, it appears from a letter which he addressed to me on the subject, was some time ago engaged in experiments on Cheshire salt. The particulars he has lost; and he retains only a general recollection of the facts, which confirms, I am happy to state, the accuracy of the results obtained by my own experiments.

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* Cheshire Report, p. 53, &c.

Lymington salt from sea-water.

At Lymington, in Hampshire, advantage is taken of the greater heat of the climate, to concentrate the seawater by spontaneous evaporation to about one sixth its bulk, before admitting it into the boilers. One kind of salt is chiefly prepared there, which most nearly resembles in grain the stoved salt of Cheshire. The process varies a little in some respects, from that which has been already described. The salt is not fished (as it is termed) out of the boiler, and drained in baskets; but the water is entirely evaporated, and the whole mass of salt taken out at once, every eight hours, and removed into troughs with holes in the bottom. Through these it drains into pits made under ground, which receive the liquor called *bittern* or *bitter liquor*. Under the troughs, and in a line with the holes, are fixed upright stakes, on which a portion of salt, that would otherwise have escaped, crystallizes and forms, in the course of ten or twelve days, on each stake, a mass of sixty or eighty pounds. These lumps are called *salt cats*. They bear the proportion to the common salt, made from the same brine, of 1 tun to 100.

Bittern.

Salt cats.

Preparation of sulphate of magnesia.

From the mother brine, or bitter liquor, which has drained into the pits, the sulphate of magnesia is made during the winter season, when the manufacture of salt is suspended, in consequence of the want of the temperature, required for the spontaneous evaporation of the sea-water. The process is a very simple one*. The bitter liquor from the pits is boiled for some hours in the pans, which are used in summer to prepare common salt; and the impurities, which rise to the surface, are removed by skimming. During the evaporation, a portion of common salt separates; and this, as it is too impure for use, is reserved for the purpose of concentrating the brine in summer. The evaporated bitter liquor is then removed into wooden coolers 8 feet long, 5 feet wide, and 1 foot deep. In these it remains twenty-four hours, during which time, if the weather prove clear and cold, the sulphate of magnesia, or Epsom salt, crystallizes at the bottom of the coolers, in quantity equal to about one eighth of the boiled liquor. The uncrystallizable fluid is then let off through plugholes at the bottom of the coolers; and the Epsom salt, after being drained in baskets, is deposited in the store-house. This is termed *single* Epsom salts; and after solution, and a second crystallization, it acquires the name of *double Epsom* salts. Four or five tuns of sulphate of magnesia are produced from a quantity of brine, which has yielded 100 tuns of common, and 1 tun of cat salt.

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* I am indebted for an account of this process, as well as of the method of making common salt at Lymington, to the liberal communication of Charles St. Barbe, Esq., of that place. Though not strictly connected

Seawater saturated with rock salt.

On the banks of the Mersey, near its junction with the Irish Channel, the water of that river before evaporation is brought to the state of a saturated brine, by the addition of rock salt. The advantage of this method of proceeding will be obvious, when it is stated, that 100 tuns of this brine yield at least 23 tuns of common salt, whereas from the same quantity of seawater, with an equal expenditure of fuel, only 2 tuns 17 cwt. of salt can be produced*.

Rock salt used for provision.

Within the few past years, an attempt has been made to apply rock salt itself to the packing of provision. For this

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connected with the subject, I give his description of the mode of making Epsom salt, because no correct statement of the process has, I believe, been hitherto published. The analysis of seawater, indeed, by a justly distinguished chemist (Bergman), excludes, erroneously, the sulphate of magnesia from its composition; and his results have led to the opinion, that, to manufacture this salt on the large scale, requires the addition either of sulphuric acid, or of some sulphate to the *bitter liquor*. (See Aikin's Chemical Dictionary, II, 388.)

Errour of Bergman.

* See the Earl of Dundonald's "Thoughts on the Manufacture and Trade of Salt." London, 1785.

purpose it is crushed to the proper size between iron rollers. The trials which have been made, I am informed, are but few, and the results hitherto are not perfectly known.

Bay salt.

The *bay salt* imported from foreign countries is well known to be prepared by the spontaneous evaporation of seawater, which, for this purpose, is confined in shallow pits, and exposed to the full influence of the sun and air. I have no addition to make to the accounts of its manufacture, which have already been given by various writers*.

Results of the examination, and analyses.

As the results of the investigation, which forms the subject of this memoir, may be acceptable to many persons, who can scarcely be expected to take an interest in a long detail of analytical processes, I shall present in the following section a general view of the experiments, and of the conclusions that may be deduced from them. In the last place, in order that other chemists may be enabled to repeat the analyses under similar circumstances, I shall describe minutely the methods that were adopted, some of which are new, and others reduced to greater precision. If however, in the future progress of science, it should appear, that any of these processes are imperfect, it may still be admitted, that, for all useful purposes, they afford a fair *comparison* of the composition of the several varieties of culinary salt; since the sources of fallacy, that may hereafter be discovered, must have been the same in every case, and have produced in each an errour of nearly the same amount.

SECT. II. *General Statement of the results of the Experiments, and Conclusions that may be deduced from them.*

Different salts compared.

A comparison of the component parts of British and foreign salts, and of different varieties of British salt with each other, will best be made by an examination of the following table, which comprehends the results of the analysis of equal weights of each variety.

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* Encyclop. Méthod. Art. Salins. (Des Marais Salans). Aikin's Dictionary of Chemistry, II, 224. Watson's Chemistry, Vol. II, p. 52. It is necessary to remark, that a great proportion of what is sold in London as bay salt is Cheshire large grained fishery salt.

Their component parts.

1000 parts by weight consist of

	Kind of salt.	Insol. matter.	Muriate of lime.	Muriate of magnesia.	Total earthy muriates.	Sulph. of lime.	Sulph. of magnesia.	Total sulphates.	Total impurities.	Pure muriate of soda.
Foreign bay salt.	St. Ube's	9	a trace	3	3	23½	4½	28	40	960
	St. Martin's	12	ditto	3½	3½	19	6	25	40½	959½
	Oleron	10	ditto	2	2	19½	4¼	23¾	35¾	964¼
British salt from salt water.	Scotch (common)	4	—	28 or +	28 or +	15	17½	32½	64½	935½
	Scotch (Sunday)	1	—	11½	11½	12	4½	16½	29	971
	Lymington (common)	2	—	11	11 or +	15	35	50	63	937
	Ditto (cat)	1	—	5	5	1	5	6	12	988
Cheshire salt.	Crushed rock	10	0·1/16	0·3/16	0·¼	6½	—	6½	16¾	983¼
	Fishery	1	0·¼	0·¾	1	11¼	—	11¼	13¼	986¾
	Common	1	0·¼	0·¾	1	14½	—	14½	16½	983½
	Stoved	1	0·¼	0·¾	1	15½	—	15½	17½	982½

I. The

Proportion of impurities.

I. The *total amount of impurities*, and the *quantity of real muriate of soda*, contained in each variety of common salt, may be learned by inspecting the last two columns of the table. From these it appears, that the foreign bay salt is purer, generally speaking, than salt which is prepared by the rapid evaporation of seawater; but that it is contaminated with about three times the amount of impurities discoverable in an equal weight of the Cheshire *large-grained* salt, and with more than twice those that are found in the *stoved* and *common* salt of the same district.

Insoluble matter.

II. The *insoluble matter* in the foreign salt, after the action of boiling water, appears to be chiefly argillaceous earth coloured by oxide of iron; and is probably derived in part from the pits, in which the seawater is submitted to evaporation. We may, perhaps, assign the same origin to the very minute portion of muriate of lime, which is not found in the salt prepared by evaporating seawater in metallic vessels, or even in the mother liquor, or uncrystallizable residue. In seasalt prepared by rapid evaporation, the insoluble portion is a mixture of carbonate of lime with carbonate of magnesia, and a fine silicious sand; and in the salt prepared from Cheshire brine, it is almost entirely carbonate of lime. The insoluble part of the less pure pieces of rock salt is chiefly a marly earth, with some sulphate of lime. The quantity of this impurity, as it is stated in the table, is considerably below the average, which in my experiments has varied from 10 to 45 parts in 1000. Some estimate of its general proportion, when ascertained on a larger scale, may be formed from the fact, that government, in levying the duties, allows 65lb. to the bushel of rock salt, instead of 56lb., the usual weight of a bushel of salt.

Earthy muriates.

III. The *earthy muriates*, and especially that with base of magnesia, abound most in salt which is prepared by the rapid evaporation of seawater. Now since common salt, in all its forms, contains, as will afterward appear, very little water of crystallization, it is probable, that the muriate of magnesia, discovered by the analysis of seasalt, is derived entirely from that portion of the mother liquor, which adheres to the salt after being drained, and which amounts to about one seventh of its weight. The larger the size of the grain, the less is the quantity of this solution, which the salt holds suspended; and hence the salt prepared at a lower degree of heat, being in larger crystals, is less debased by the magnesian muriate, than the salt formed at a boiling temperature. It is probable also, that, when the salt is drawn at intervals from the boiler, the proportion of the earthy muriate will vary with the period of the evaporation, at which it is removed. For it may readily be conceived, that as the proportion of the earthy muriates in any brine is increased by the separation of muriate of soda, the greater will be the quantity of these muriates, which the crystals of common salt, formed in the midst of the brine, will retain. It follows therefore, that, so far as the earthy muriates only are concerned, salt must diminish in purity as the process of evaporation advances.

Cheshire salt.

In the several varieties of Cheshire salt, the earthy muriates do not exceed one thousandth part of their weight, and they are precisely (or so nearly so, that the difference is not ascertainable) the same in all. This will cease to be matter of surprise, when it is considered, that the salt obtained by evaporating to dryness the whole of a portion of Cheshire brine does not give more than 5 parts of earthy muriates in 1000. In the entire salt of seawater, according to Bergman, the earthy muriates form no less than 213 parts in the same quantity.

Pure muriate of soda attracts moisture.

According to the proportion in which the earthy muriates are present in any kind of salt, will be its power of deliquescence, or of attracting moisture from the atmosphere. It is not entirely, however, from the salts with earthy base, that common salt derives this quality; for the most transparent specimens of rock salt, which I find to consist of absolutely pure muriate of soda, attract much moisture from a humid atmosphere.

Earthy sulphates.

IV. The *sulphate of magnesia* and the *sulphate of lime* both enter into the composition of all the varieties of salt prepared from seawater; but the sulphate of lime alone is found in Cheshire salt. The proportion of sulphate of magnesia is greatest in that variety of seasalt, which has been formed by rapid evaporation. In foreign bay salt, its quantity is very insignificant.

Sulphate of lime.

From the table it may be seen, that the proportion of sulphate of lime is greater in foreign bay salt, than in any variety of British salt, even than in those which are prepared from seawater with a boiling heat. The only explanation of this fact that occurs to me is, that, during the rapid evaporation of seawater, a considerable part of the calcareous sulphate is precipitated at an early stage of the process, and is partly removed in *clearing* the boiler; a process, which can scarcely be performed during the formation of bay salt in pits, the sides of which are composed of moist clay. The remainder of the selenite, thus precipitated by the rapid evaporation of seawater, enters into the composition of the pan-scale.

Same kinds of salt differ.

In the course of this inquiry, I was induced to repeat the same experiments several times, on various specimens of salt bearing the same designation; and was surprised to find, that the results by no means corresponded. In one instance, for example, fishery salt was found in 1000 parts to contain no less than 16 parts of sulphate of lime; while another specimen, nominally the same, contained only 11¾ parts of selenite in the same quantity; and a third only 5¼. At length it occurred to me, that these differences were probably owing to the circumstance of the salt having been taken from the boiler at different periods of the evaporation. I requested, therefore, to be furnished with specimens of salt, drawn at different stages of the process, from a given portion of brine, evaporated in the same boiler. These were submitted to analysis; and the results are shown in the following table.

		Sulphate of lime.
Common salt drawn from the boiler, 2 hours after the first application of heat	Contained in 1000 parts.	16
Salt drawn 4 hours after do.		11
Salt drawn 6 hours after do.		3¼

First made salt contains most sulphate of lime,

Hence it appears, that there was a gradually increasing purity in the salt from sulphate of lime, as the process of evaporation advanced; the greatest part of this earthy compound being deposited at an early stage of the process. Different specimens of the same kind of salt may, therefore, differ in chemical purity as much from each other, as from other varieties. But when the impurities, contained in a solution of muriate of soda, are of a different species from those of Cheshire brine, and consist chiefly of the earthy muriates, the order will be reversed, and the purest salt, as I have already suggested, will be that which is first deposited; the contamination with the muriate of lime or of magnesia continuing to increase, as the process advances to a conclusion*.

and least earthy muriate.

At an early period of the inquiry it appeared to me probable, that the differences between the several varieties of culinary salt might depend, in some degree, on their containing variable proportions of water of crystallization. It was found, however, by experiment, that the proportion of water in any variety of common salt, after being dried at 212° Fahrenheit, is not much greater or less than that which is contained in any other variety. Pure transparent rock salt, calcined for half an hour in a low red heat (= 4° or 5° of Wedgwood's pyrometer), lost absolutely nothing of its weight, It is remarkable, also, that the pure native salt, if free from adventitious moisture, may be suddenly and strongly heated, with scarcely any of that sound called *decrepitation* †, which is produced by the similar treatment of all the varieties of artificial salt. Even these varieties, however,

Water of crystallization nearly similar in all salt, dried at the same heat.

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* I cannot on any other principle explain the considerable differences, as to the proportion of muriate of magnesia, that were discovered in the several varieties of Scotch salt, sent to me by Dr. Thomson. For this reason, in stating the analysis of Scotch salt, I have given, in the table, that result which was most frequently obtained; and have withheld the names of the manufacturers, because the differences were probably in a great measure accidental, and not the result of greater or less skill in the preparation. One specimen of Lymington salt, which I examined, contained fully as much muriate of magnesia as any of the Scotch samples. The *cat salt* of that place, however, contrary to my expectation, proved to possess a very extraordinary degree of purity; a fact of which I satisfied myself by repeated experiments.

† Decrepitation is occasioned by the sudden conversion into vapour of the water contained in salts, when its quantity is insufficient to effect the watery fusion. It is a property peculiar to salts which hold only a very small proportion of water in combination, as muriate of soda, nitrate of lead, and sulphate of potash.

Decrepitation.

however, exposed during equal times to a low red heat, do not lose more than from half a grain to three grains in one hundred. This comparison cannot be extended to the salt prepared at a boiling temperature from seawater; because the muriate of magnesia, which these varieties contain, is decomposed at a red heat, and deprived of its acid.

The following table shows the quantity of water contained in several kinds of salt, inferred from the loss which they sustain by ignition during equal times, after being first dried at 212°.

Proportions of this water.

100 parts of large grained fishery salt contain of water	3
100 ...... foreign bay salt (St. Martin's)	3
100 ...... ditto .... (Oleron)	2½
100 ...... ditto Cheshire common salt	1¼
100 ...... ditto .... stoved salt	0¼

The loudness and violence of the decrepitation was, as nearly as could be judged, in the same order, and was most remarkable in the large grained varieties.

Proportions of real muriate given by nitrate of silver.

To determine the proportions of real muriate of soda in those varieties of artificial salt which are nearly free from earthy muriates, I employed also the process of decomposition by nitrate of silver. The following are the quantities of fused *luna cornea*, obtained from 100 grains of each of three varieties dried, previously to solution, at the temperature of 212° Fahrenheit.

100 gr. pure transparent rock salt gave of luna cornea	242
100 .. stoved salt, remarkably pure	239
100 .. fishery salt, ditto	237*

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* From 100 grains of pure artificial muriate of soda, previously heated to redness, Dr. Marcet has since informed me, that he obtained 241·6 grains of fused luna cornea. The weights of the precipitates thrown down in my experiments by nitrate of silver are not, I am aware, exactly those which might have been expected from the table of the comparative proportions of water given in the text. Each experiment, however, was twice repeated with every precaution I could adopt, and with the same results. That different kinds of salt give different proportions of luna cornea, is proved also by comparing the experiment of Dr. Marcet with the results of Dr. Black and Klaproth, both of whom found the fused muriate of silver, from 100 parts of common salt, to weigh 235 grains.

The

Proportions of ingredients, impurities excepted, nearly the same in all.

The proportion of ingredients in the several kinds of muriate of soda (setting apart the impurities) appears, therefore, to be nearly the same in all. And as the very minute quantity of water, discovered by analysis, is not constant in the several varieties, it may be inferred to be rather an accidental than a necessary ingredient; for in the latter case an invariable proportion might be expected, conformably to the important law, establishing a uniformity in the proportions of chemical compounds, which has been explained by Mr. Dalton, and confirmed by Drs. Thomson and Wollaston.

What is the cause of the difference of quality in salt?

Not the chemical composition,

but the size of the crystals.

What then, it may be inquired, is the cause of those differences, which are acknowledged, on all hands, to exist among the several species of muriate of soda, so far as respects their fitness for economical purposes? If I were to hazard an opinion, on a subject about which there must still be some uncertainty, it would be, that the differences of *chemical composition*, discovered by the preceding train of experiments, in the several varieties of culinary salt, are scarcely sufficient to account for those properties, which are imputed to them on the ground of experience. The *stoved* and *fishery* salt, for example, though differing in a very trivial degree as to the kind or proportion of their ingredients, are adapted to widely different uses. Thus the large grained salt is peculiarly fitted for the packing of fish and other provision, a purpose to which the small grained salts are much less suitable. Their different powers then of preserving food must depend on some mechanical property; and the only obvious one is the magnitude of the crystals, and their degree of compactness and hardness. Quickness of solution, it is well known, is pretty nearly proportional, all other circumstances being equal, to the quantity of surface exposed. And since the surfaces of cubes are as the squares of their sides, it should follow, that a salt, the crystals of which are of a given magnitude, will dissolve four times more slowly than one, the cubes of which have only half the size.

Practical application.

That kind of salt then which possesses most eminently the combined properties of hardness, compactness, and perfection of crystals, will be best adapted to the purpose of packing fish and other provision; because it will remain permanently between the different layers, or will be very gradually dissolved by the fluids, that exude from the provision; thus furnishing a slow, but constant supply of saturated brine. On the other hand, for the purpose of preparing the pickle, or of *striking* the meat, which is done by immersion in a saturated solution of salt, the smaller grained varieties answer equally well; or, on account of their greater solubility, even better.

Specific gravity.

With the hardness or strong aggregation of the several varieties of salt, it seemed to me not improbable, that their specific gravity might, in some degree, be connected. The exact determination of this property in saline substances is, however, a problem of considerable difficulty, as will sufficiently appear from the various results which have been given, with respect to the same salts, by different experimentalists. Thus Muschenbroek makes the specific gravity of artificial muriate of soda to vary from 1918 to 2148, the mean of which is 2033. Sir Isaac Newton states it at 2143, and Hassenfratz at 2200*. All that was necessary for my purpose was an approximation to the truth; and the introduction of a small errour could be of no importance, provided it were the same in every case, since the comparison would still hold good.

Rock salt.

The specific gravity of rock salt there can be little difficulty in determining with precision. A piece of this salt†, of such perfect transparency, that I had reserved it as a cabinet specimen, weighed in the air 513 grains, and lost, when weighed in alcohol, 194 grains. The alcohol, at the temperature of 56° Fahrenheit, had the specific gravity of 820, and hence that of the salt may be estimated at 2170. Another specimen considerably less pure, and more approaching to a fibrous fracture, had the specific gravity of 2125 only.

Mode of ascertaining the specific gravity of different kinds of salt.

For ascertaining the specific gravities of artificial varieties of salts, I used a very simple contrivance. It consisted of a glass globe about 3½ inches diameter, having a stem or neck 10 inches long. Sixteen cubic inches of water (each 252½ grains at 60° Fahrenheit) filled the whole of the globe, and about half an inch of the lower part of the neck; and from the line where the water stood in the instrument, it was accurately graduated upwards into hundredth parts of a cubical inch. Into this vessel I poured exactly sixteen cubic inches of a perfectly saturated solution of common salt; and then added 400 grains of the salt under examination, washing down the particles that adhered to the neck by a portion of the liquid, which had been previously taken out of the globe for the purpose. As much as possible of the air which adhered to the salt was dislodged by agitation; and the increase of bulk was then observed.

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* Annales de Chimie, vol. XXVIII, p. 13.

† Foliated rock salt of Jameson. See his Mineralogy, vol. II, p. 10.

Care was taken, that the salts were all of equal temperature and dryness, and that no change of temperature happened during the experiment.

	Hundredths of a cub. in.	Hence its specific grav. was*	Specific gravity of salt.
400 grains of the less pure kind of rock salt, broken down into small fragments, filled the space of	75	2112	
400 grains of stoved salt............	75	2112	
400 do. (another sample)	76	2084	
400 do. common salt............	76	2084	
400 large grained fishery salt........	83	1909	
400 do. (another sample)	83	1909	
400 St. Ube's	82	1932	

If the above mode of determination at all approach to correctness, it would appear, that the specific gravity of rock salt is diminished, by being broken into small fragments, from 2125 to 2112, probably in consequence of the quantity of air which the fragments envelope, and which cannot be entirely separated by agitation. From the numbers given in the last column, it is evident, that the smaller grained salts are specifically heavier than those which are composed of larger and more perfect crystals. A difference of only one or two hundredth parts of a cubic inch is perhaps entitled, in a process of this kind, to little reliance; and I do not therefore regard it as indicating any material difference in the specific gravity of the first four, or last three salts submitted to experiment. But when the difference amounts to eight hundredths, as between the small and large grained salt, it may safely be imputed to an inferior specific gravity in that species, which occupies so much greater a proportional bulk*.

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* Distilled water at 1000 being taken as the standard.

British salt at least equal to foreign.

The last series of experiments proves decisively, that in an important quality (viz. that of specific gravity), which is probably connected with the mechanical property of hardness and compactness of crystals, little or no difference is discoverable between the large grained salt of British, and that of foreign manufacture. If no superiority, then, be claimed for British salt as applicable to economical purposes, on account of the greater degree of chemical purity which unquestionably belongs to it, it may safely, I believe, be asserted, that the larger grained varieties are, as to their mechanical properties, fully equal to the foreign bay salt. And the period, it may be hoped, is not far distant, when a prejudice (for such, from the result of this investigation, it appears to be) will be done away, which has long proved injurious to the interests and prosperity of an important branch of British manufacture.

(To be concluded in our next.)

IX.

On the Proportions of the Elements of some Combinations, particularly of the Alkaline Carbonates and Subcarbonates: by Mr. J. E. Berard†.

Component parts of salts should be ac-

"THE accurate determination of the component parts of saline substances is of the more importance, because it is curately determined.

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* Mr. Hassenfratz seems to have suspected, that a difference in the specific gravity of the same salt may be occasioned by a variation in its mode of crystallization. *De la Pesanteur spécifique des Sels*, Ann. de Chim. XXVIII, p. 17.

† Abstracted from Ann. de Chimie, vol. LXXI, p. 41.

is employed as the basis of other chemical analyses. Berthollet, who has sought to determine some of these in his late papers, was desirous, that they should be carried to the highest degree of accuracy; and invited me, to resume the subject, reiterating the experiments, varying the methods, and taking the greatest care to avoid every source of errour. This I have endeavoured to do, and at the same time I extended my observations to a greater number of compounds.

"As these determinations depend particularly on the accuracy of the weights, I think it necessary to say, that I always used a balance made by Fortin, which, when loaded with a kilogramme [2¼lbs. nearly], is sensible to a milligramme [0·01544 of a grain, or one millionth of the load]."

After this introduction, Mr. Berard, in a pretty long paper, gives the detail of his experiments; but as these would occupy much room to little purpose, I shall pass them over, merely giving the tabulated results, with which he concludes.*

Table of salts.

Salts.	*Base.*	*Acid.*	*Total.*
Muriate of potash - - - - - -	66·66	33·34	100*
——— of soda - - - - - - - -	57·00	43·00	100
Sulphate of barytes - - - - - -	67·70	32·30	100†
——— of potash - - - - -	57·24	42.76	100
——— of soda - - - - - - -	47·22	52·78	100
Nitrate of potash - - - - - -	48·64	51·36	100
Carbonate of potash - - - - -	53·81	46·19	100‡
——— of soda - - - - - -	44·38	55·62	100
Subcarbonate of potash - - -	70·21	29·97	100
——— of soda - - - -	62·53	37·47	100

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* Observations on the Proportions of the Elements of Compounds, Mém. d'Arcueil, vol. II.

† Ibid.

‡ I suppose the carbonate of potash, as well as the other salts, to be totally deprived of water.

X.

X.

On Chemical Printing, and particularly on the Progress of this Art in Germany: by Mr. MARCEL DE SERRES, *Inspector of Arts, Sciences, and Manufactures**.

Vienna, october the 17*th*, 1809.

istory of the art.

THE art of printing from stone, known in Germany by the name of *chemische druckerey*, "chemical printing," originated in Germany; whence it spread first into England, then into Italy, and lately into France. It was invented by Aloys Senefelter, who was born at Prague, in Bohemia. Nine years ago he obtained of the king, formerly elector of Bavaria, an exclusive patent for its use for thirteen years; but he afterward sold the right to his brothers. Some time after Senefelter sold his right also to Mr. Andrew von Offenbach, who at present exercises the art in England. In 1802 he came to Vienna, to solicit a patent, and in 1803 he obtained one from the emperor of Austria for ten years. Changing his mind, he parted with this patent to Messrs. Steiner and Krasnitzki, returned to Bavaria, and set up a chemical printing office at Munich in partnership with some other persons. Messrs. Steiner and Krasnitzki still continue the business at Vienna, under the patronage of the counsellor of regency Startl von Luchsenstein, who is a zealous promoter of every useful undertaking.

At the chemical printing office at Munich the art has attained the greatest perfection, that of Stutgard apparently being of much less importance. Mr. Chauvron was the first who obtained a patent in France for printing or engraving on stone, and Mr. Guyot-Desmarets did not attempt it till after him.

The processes employed are simple, but as only a brief account of them has yet been given, it may be of use at least to make known those followed in Germany.

Three different methods.

In the chemical printing office at Vienna three different methods are employed; but that termed in relief is most frequently used. This is the general mode of printing music.

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* Abridged from the Annales de Chim., vol. LXXII, p. 202.

The second method is the sunk. This is preferred for prints.

The third method is the flat, or neither raised nor sunk. This is useful for imitating drawings, particularly where the impression is intended to resemble crayons.

The stone used.

For printing or engraving in this method a block of marble is employed, or any other calcareous stone, that is easily corroded, and will take a good polish. It should be two inches or two inches and half thick, and of a size proportioned to the purpose for which it is intended. A close texture is considered as advantageous.

Process described.

When the stone is well polished and dry, the first step is to trace the drawing, notes, or letters, to be printed, with a pencil. The design is not very conspicuous, but it is rendered so by passing over the strokes of the pencil a particular ink, of which a great secret is made. This ink is made of a solution of lac in potash, which is coloured with the soot from burning wax. This appears to be the most suitable black for the purpose. When the design has been gone over with this ink, it is left to dry, which commonly takes about two hours; but this depends much on the temperature and dryness of the air.

After the ink is dry, nitric acid, more or less diluted according to the degree of relief desired, is poured on the stone; and corrodes every part of it, except where defended by the resinous ink.

Mode of printing.

The block being washed with water, an ink similar to that commonly used for printing is distributed over it by means of printers balls, a sheet of paper disposed on a frame is laid on it, and this is pressed down by means of a copper roller, or copper press. The beauty of the impression will necessarily depend on that of the design. These copper presses are very ingeniously constructed in Germany, and easily worked. Their weight is proportional to the method of printing used.

The stone may be used for other works.

When the desired number of impressions is taken off, and the work is not intended to be usedany more, the stone is polished anew; and thus it may be made to serve for thirty or forty different works.

Chalk or sunk method.

The sunk, or chalk method differs from that termed in relief only in having the stone much more corroded by the nitric acid. This is chiefly employed for prints, and has the advantage of remedying that uniformity of tint, which is common to prints from the chemical press. It is natural, that the higher parts should take less of the ink, and the lower parts more, so that the impression has less monotony; a defect hitherto seemingly inherent in this mode of printing. For this method too the rollers must be stronger and heavier.

Its use.

As this method is more expensive, it is given up: yet for prints, where some degree of effect is required, and more clearness, it is to be preferred. In this method nearly pure nitric acid is employed. Indeed when the art was first invented, pure nitric acid was always used; but soon after, to save expense, it was diluted with water; and since that it has been employed more or less diluted, according to the effect wished to be produced on the stone.

Relief.

For the method in relief, as it is called, nitric acid with half water is used.

Flat method.

In the flat method less nitric acid is used. It is not to be supposed, that the surface is quite plain in this way; but the lines are very little raised, so that they can scarcely be perceived to stand above the ground but by the finger.

Application.

The works executed in stone are; 1, imitations of wood cuts: 2, imitations of engravings in the dotted manner: 3, drawings: 4, music: 5, all kinds of writing: 6, maps: 7, copperplates.

Advantages.

The advantages of this method are, that it has a peculiar character, which cannot be represented by any other mode, while it gives a tolerable imitation of other methods; and still more the celerity, with which it can be executed. A subject, that an artist could not finish in five or six days on copper, may be engraved on stone in one or two. While a copperplate printer is taking off six or seven hundred impressions, two thousand may be printed in this way. A copperplate will scarcely give a thousand good impressions, while in this way several thousands may be taken off, and the last be as perfect as the first. Thirty thousand have been taken

Celerity.

Number of impressions.

off one design at Vienna, and the last was as beautiful as the first. This is intended to be carried still farther, for the purpose of printing bank notes. The most expert music engraver can scarcely execute four pages of music on pewter in a day, but the engraver on stone can finish twice as much in that time. Bank notes.

To enter into the particulars would take up too much room, but experience has shown, that this mode saves two thirds of the expense of engraving on copper or pewter. Cheapness.

After having mentioned the advantages of printing from stone, it is juſt to point out its disadvantages. These are, the difficulty of giving that diversity of tone, which is admired in engravings. Thus for instance, the finest prints that this art has yet produced are unquestionably those, that have been executed at Munich* from those celebrated drawings, which from a whim, in which painters are apt enough to indulge, Albert Durer made in a prayerbook. These prints are executed with spirit, and the stroke is frequently clean; but it is uniform, so that the print is somewhat gray and monotonous. The difference is still better perceived, on comparing these prints with those etched by the different masters themselves. Disadvantages. Best specimens.

The same inconvenience is found in music, the uniformity that prevails rendering the music less easy to read.

We must not too hastily conclude however, that this new art is not important: we should endeavour to find means of remedying the inconveniences, that appear to arise from the mode employed. If such means be discovered, which we may hope from experience showing, that the manner of applying the acid and of drawing upon the stone are the points most important to improve, this mode of printing will combine a saving both of time and expense. The great number of copies too, that may be taken off, is not one of its leaſt advantages. Improvement to be attempted.

It remains now to notice the differences, that appear to exist in the chemical printing offices of different cities. At Milan a little nitric acid is poured over the stone, as at Differences in different places.

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* Albrecht Durers Christlich mythologische Handzeichnungen. Strixner, Munich, 1808. Different inks have been used for prints, as black, red, violet, and green.

Vienna: but it is said, that they cannot take off above five hundred impressions. This must be owing to the nature of the stone employed, which is procured from Verona.

nch mode. Chauvron, the first who set up a chemical printing office at Paris, after having traced the design on stone with a resinous ink, merely wets it with water, and wipes off the water from the design. Printing ink is then applied by beating in the common way with balls; and, as this does not adhere to the wet stone, the resinous strokes only produce an impression. Chauvron is said to have printed a great deal of music in this way.

Use of nitric acid. We must observe, that, where nitric acid is not used, the prints will never be so fine, and so many impressions cannot be taken off. The use of nitric acid therefore cannot be too strongly recommended.

XI.

Observations on Dr. Bostock's *Remarks upon Meteorology, by* Luke Howard, *Esq.*

To WILLIAM NICHOLSON.

Respected Friend,

Natural history and nomenclature of the clouds. It is about eight years since I brought forward an essay containing the Natural History of the Clouds, with a nomenclature formed chiefly from their visible structure. This work, imperfect as it was, met with a favourable reception. In the Annual Review for 1804, Dr. Bostock bestowed upon it a pretty long critique, on the whole favourable to the adoption of the nomenclature, but which he seems to have since forgotten. He now says, "I am not unaware, that a scientific nomenclature for the appearance of the clouds has been attempted by Mr. Howard, but I hope I shall not be accused of presumption, if I give it as my opinion, that his set of names is much too confined to be of any great use, and that the hypothesis on which he proceeds is not entirely correct."

Nomenclature objected to.

There

This free for any one.

There was surely no occasion to deprecate censure. In common with the whole public, Dr. B. has just the same liberty to reject the nomenclature, which I had to propose it; and from the care with which he avoids using a single term of mine, I perceive he intends to avail himself of this. Should it prove, that it is to make way ultimately for something of his own, I hope to entertain his attempt with the impartiality which the interests of science require. In the mean time I may be allowed to say something in behalf of my system, which I shall do the more readily, as it will give an opportunity of meeting his former objections.

The system explained.

The system in question imposes names on *seven* modifications of cloud, only: but these divisions are, in effect, so many genera, some of which comprehend many species; if indeed it be right to apply these terms to the evanescent forms of our subject, which occur at uncertain and sometimes very distant intervals, and run through gradations so delicate, and combinations so varied, that the leisure of a long life might not more than suffice accurately to observe and describe them. In the interval since 1804, I have not seen sufficient cause to disturb my original plan by adding or suppressing modifications. It is not that I am vain enough to deem it perfect, but I believe it answers the intended purpose.

Multiplication of terms not desirable,

In the present infant state of meteorology, and until these or some other general distinctions shall be adopted and practically understood, I do not apprehend it will be of advantage to multiply terms, especially such as, being referable to no system, are applied to complicated appearances, which must be analysed before they can be clearly understood.

till more specific distinctions are noted,

The materials, however, for specific distinctions, will accumulate in proportion as accurate observers increase, and it is matter of indifference to me, who firſt imposes appropriate names on well ascertained and well defined species or combinations.

Mr. Dalton's hypothesis

As to the erroneous hypothesis, on which I am said to proceed, it must be that of Dalton on the constitution of the atmosphere, which I used in aid of my explanations, or rather *speculations*, on the causes of the phenomena. If Dr. B. will turn to the article *Cloud*, in Rees's Cyclopædia, not essential to the natural history.

he will there find the natural history detached from this so much combated hypothesis, and resting solely on its proper basis, that of long continued and attentive observation of the phenomena, I undertook to describe and classify.

The terms of Dr. Bostock only objectionable.

In objecting, as I shall next be obliged to do, to the whole method of Dr. Bostock in forming his terms, I would not be understood to detract from the value of his original observations. A continuation of them in more clear and definite phraseology cannot fail to prove useful to the science. To use new terms is in effect to propose them for use, or to introduce a new nomenclature. Those of Dr. Bostock are liable to objections more apparent than that of being "uncouth;" they are inaccurate and imperfect. To make this appear we must examine them singly, which will also give opportunity for the discussion of several facts, to which they relate: and first, the *arc*, past, present, and future.

The arc.

This particular formation of the clouds must have attracted the notice of most observers of the sky. The structure is not very clear from the Doctor's account. He says, the lines converge to a point in the horizon, and such is certainly the appearance to the eye. In the article *Cloud* before mentioned, the reader will find it thus noticed: "It (the cirrus) is sometimes spread horizontally through a vast extent of atmosphere; the whole breadth of the sky being insufficient to show where it terminates. In this case, its *parallel* bars appear, by an optical deception, to converge in opposite points of the horizon." These parallel bars are the *linear* arc of Dr. B. When the cirrus is compound, we have his *feather'd* arc; and in varieties of the cirrocumulus, the *mottled* and *wreathed* arc. Now, although each of these linear clouds may, in some sense, be an arc, as following the curve of the Earth's surface, yet it is not this curve which the eye perceives, but a system of converging lines. The term *arc* therefore, applied to such a system, seemed so unphilosophical, that I could not imagine whence it came; till I remembered that the name of Noah's *ark* is given, in popular language, to an assemblage of clouds, the nature of which is not very obvious from the name. Recollecting this I was led to suppose, that Dr. B. had borrowed the sound, though not the sense, of this popular allusion to the orderly *converging march* of the animals assembling to Noah. Be this as it may, the observations on a connection between this phenomenon and the variations of the wind are highly interesting. In verifying them by future cases it will be proper to ascertain a fact, which seems to have been taken for granted: viz. that the lines of cloud continue to range between the same points, while the wind below is veering: for it is possible, extensive as they are, that they may undergo a change of direction. The phenomenon is rare, in this champaign district, and I believe frequent near the sea, or among mountains. I must protest, convenience notwithstanding, against calling a thing *past* or *future*, which is *present*, and the subject of actual observation.

Revolving, & reverse revolving day.

A *revolving day.* A *reverse revolving day.* These are not more happily formed. A reader meeting with the latter, and not having the vocabulary at hand, might attempt to solve the puzzle by considering, that day and night are produced by a revolution of the Earth on its axis. A *reverse* revolving day must then be one in which, that revolution being reversed, the sun had risen in the west and set in the east! To the terms a direct or a reverse revolution of *the wind* there can be little objected. These are prognostics of long standing, and like others, taken singly, are not infallible. We had a *reverse revolution* here on the 4th inst. (as I recollect) followed by a dry, though cloudy day, with an easterly current.

Sour day.

The term *sour day*, I think, we may leave to the painters. It is matter of taste rather than of science, to define it.

Dissolving air,

Dissolving air or *constitution* is obviously connected with an hypothesis, and that an exploded one. Solution, in its only proper sense (as the result of a chemical attraction between the ponderable base of air and water) has been found to enter for little or nothing into the case of evaporation.

Various epithets of clouds.

Round clouds, shaded clouds, piled clouds, rolling clouds, white clouds on a gray ground, &c. I do not see the advantage to science of these attempts to substitute description for definition. The *piled* cloud will be the *shaded*, or some other, when it comes toward the zenith; and the *shaded* will be light in the horizon opposed to the sun. As for the *rolling* cloud I have not yet detected it; and it seems too poetical,

cal, if, as I conjecture, it is so named, because its parts, if solid, would roll when on an inclined plane. There is a rapid introversion or *curling* of the tops of the cumulus while the cloud is rapidly evaporating, or rather perhaps *dispersing*, which is a pretty good indication of approaching rain; but the *mass* in this case continues well balanced.

Cumulus.

I shall here repeat (in opposition to the assertion of Dr. Bostock, Annual Review, 1804, p. 900,) that the cumulus *is a production of the day*; and that it is not "frequently," but on the contrary very rarely "seen in the night." He may have taken for it the remnants of an evening cumulostratus, or the parts of a dense and low cirrocumulus, as they often appear by moonlight; but if he will take up again the definition, and repeat his observations, I think he will not meet with it after sunset, save by chance in a thunder group.

Tufts, lines, flocks.

Tufts, lines, flocks. These are varieties of the cirrus; but cirrus is a *Latin* term, and to such, it seems, the Doctor has at present a strong objection, founded on this reason, that the farther improvement of the science will probably, in a considerable degree, depend upon the observations of the *unlearned* (An. Rev. 1804, p. 898). But surely the unlearned can learn, as they have done heretofore. *Alphabet*, which is greek curtailed, is as well understood as *a, b, c*; *zenith* and *nadir* are Arabic; and as for *Latin*, our Scotch gardeners can talk it fluently (I will not say classically), and we have senior, junior, maximum, minimum, prospectus, index, finis, quota, quantum, vacuum, and a hundred more, which the unlearned use daily, without suspecting how learned they are.

Advantage of terms, the meaning of which must be learned.

There is a great advantage, ultimately, to the learner, however small his capacity, in giving him terms, the use of which muſt be acquired with some labour by means of definitions. It packs his knowledge, if I may be allowed the phrase, in much less room, and enables him to find it with ease when wanted. But I had another motive, of still greater importance, to the commencement of a latin nomenclature.

A latin nomenclature intelligible on the continent.

I conceived, that the subject would interest observers on the continent; and that, by means of a language common to all, the observations of each country might with ease be communicated to the rest; which they never could be, with accuracy, by such a nomenclature as I have been examining. I hope, therefore, that my fellow-labourer, whose production I have, in my turn, been criticising, will accede to this principle, that, in forming a nomenclature for meteorology, the terms which it shall be needful to establish and define (in addition to common words on the subject) shall be carefully formed from the Latin, in which there exist already a number of words that are perfectly apposite. Those who are precluded from acquiring these, may be left, as in other sciences, to the use of their peculiar synonyms, which the student will find it no very difficult task to attach to their correspondent terms in his own series; for, if I have a just conception of the extent of the vulgar nomenclature, it would form but a limited collection.

Transparent atmosphere.

I shall not now enter into every consideration, which has occurred on the perusal of Dr. Bostock's elaborate communication; but before I conclude I must notice another phenomenon, in the explanation of which I differ from him. He speaks of a *transparent* condition of the atmosphere, as one of the most infallible signs of a change of weather, and as seldom lasting more than a few hours. I am convinced from several points in his description, and particularly from the *varnish* which it is said to throw upon any large expanse of water, that he intends a condition of the atmosphere, in which, though transparent below, it is more opaque than common in the higher part. There exists in those regions at such times a quantity of water in a peculiar state of diffusion, giving a strong milky opacity to the twilight, the reflection of which occasions the varnished appearance to which he adverts. The whole quarter above the sun, after it has set, has sometimes even a lively *pink* tinge, and in this case a thunder storm ensues. The *lilac* or *violet* band, spread round the horizon, is merely the colour of the falling dew; and this belongs rather to that perfectly transparent state of the atmosphere, which accompanies our easterly winds in the spring, and is so far from being transient, that there is no state in which it is found to continue for a longer time the same.

I am, respectfully, thy friend,

LUKE HOWARD.

Plaistow, Essex,
16th of 6th Month, 1810.

XII.

XII.

Experiments on the comparative Powers of Cylinder and Plate electrical Machines, and on a means of doubling, trebling, or quadrupling their charging Power: by Mr. JOHN CUTHBERTSON *and Mr.* G. J. SINGER. *Communicated by Mr.* SINGER.

Best shape for electrical machines doubted.

Formerly but slight effects produced.

Improvements.

THE opinion of electricians has been at various times much divided with regard to the best form of an electrical machine. Globes, spheroids, cylinders, and plates, have been alternately employed and recommended by various experimenters: but the last two forms have been recently by far the most prevalent. The earlier electricians produced but slight electrical effects; and though some attempts were made at improvement, by alterations in the structure of the apparatus, and by the adoption of machinery; the tests of electrical action at that time known were far from adequate to any accurate comparison of their relative merits. The much improved construction of the cylinder machine by Mr. Nairne, the experiments on electrical excitation by Mr. Nicholson, and the structure of the unrivalled Harlem apparatus by Mr. Cuthbertson, are the circumstances that have most enlightened this subject; and it is from these sources, the various opinions, that have been entertained in this respect, have principally originated.

Effects influenced by slight circumstances.

These attended to in the following experiments.

The influence of the hygrometrical state of the atmosphere on electrical experiments, and the considerable diversity of effect produced by slight alterations in the disposition of the apparatus, will convince every electrician, that no just comparison of the merits of any two instruments can be made, unless they are employed in one situation, at the same time, and under similar circumstances. These essential requisites to just observation have been attended to as strictly as possible in the following experiments, made with the express intention of obtaining every useful information on this particular subject.

Instruments employed.

The instruments employed for comparison were a cylinder of 14 inches diameter (with a multiplying wheel and pulley, the proportionate diameter of which are as four to one), and a single plate machine of 24 inches diameter, turned by a single winch, as usual. The management and excitation of each machine were undertaken by the individual who had constructed it, and an equal advantage was thus afforded to both. To ensure greater accuracy, the results noted were in all cases the mean result of repeated experiments; without this precaution, the sources of anomaly would be much more frequent and numerous.

Best test of electrical action.

Mr. Cuthbertson's experiments (published in this journal, 4to edition, vol. II, page 525: and in his Essay on practical Electricity and Galvanism, parts 6 and 7) have decisively proved, that no known test of electrical action is so uniformly accurate, as that of the gradual increase of charge, conveyed to a known measure of coated surface, measured by his electrometer, and by the fusion of determinate quantities of wire. These were consequently the means most frequently employed, and on which the greatest reliance was placed; but in the progress of inquiry recourse was also had to every known means of measuring comparative quantities.

But every known means here employed.

Difficulties at first experienced surmounted.

In our first experiments much difficulty was experienced, in consequence of the variation produced in charges of high intensity, by the different arrangement of the conductors, (a circumstance which cannot be avoided in the present form of the instruments): but by altering the size and situation of the terminating balls, and modifying the charge, the production of tolerably uniform results was at length accomplished. The mean result of nearly one hundred distinct trials tended to prove, that the charging power of the two instruments was precisely equal; and this conclusion was rendered indisputably accurate by the following experiments.

The two machines of equal power, proved by experiments.

Exp. 1. A battery of 15 jars, exposing about 17 square feet of coated surface, was connected with Mr. Cuthbertson's electrometer, the slider on the arm placed at 15 grains. 4 feet of Iron wire, $\frac{1}{160}$ of an inch diameter, were placed in the circuit. The battery was firſt charged by the cylinder. 130 turns of the wheel effected the discharge, the wire was rendered red hot and fused into balls.

Exp. 2. The same arrangement as in the former experiment. The charge communicated by the Plate. The discharge was produced by 138 turns, and the wire fused as before.

Exp. 3. The apparatus arranged as in the former experiments. The *two* machines employed together to communicate the charge. The electrometer discharged itself in 65 turns; the wire was melted as before.

The effects equal,

In this last experiment the effect was produced in exactly half the number of turns by the *two* instruments, that had been required by the most favourable action of *one*; and it is perhaps impossible to conceive a more complete proof of the similarity of their powers. The 48 inches of wire fused in these experiments was afterward found to be a tolerably exact measure of the force of the charge employed; for on repeating the experiment with 49 inches of the same wire, it was barely rendered red hot.

and the wire fused a good measure of the charge.

The plate machine more easily worked.

Equal effects being produced by either machine in equal times, it became necessary to ascertain, whether any difference existed in the power required to put them in motion. The handles were placed in a horizontal position, and weights applied to them. 8lbs. troy were sufficient to move the handle of the plate machine, but it required 14lbs. to produce the same effect with the cylinder.

Some additions necessary.

Here our first series of experiments terminated; the results we had obtained had shown the necessity of some addition to our apparatus, and we mutually agreed to defer the farther prosecution of the subject, till these should be supplied. In the mean time, some circumstances, which had occurred in these experiments, continued to occupy our consideration separately. Mr. Cuthbertson, as well as myself, had been much surprised at the uniform action of the two instruments; and the active discrimination with which he considered the subject soon offered a means of increasing and extending their powers, to an almost indefinite extent.

Construction of the cylinder.

The cylinder machine we employed is constructed on a plan I believe to be peculiar to myself; it is entirely insulated, in the manner of Mr. Nairne's; but the motion is communicated by multiplying wheels with a silk cord, instead of an insulating winch. I adopted this plan to prevent the effect of that undulation, which has been described by Mr. Nicholson as peculiar to cylinders (in consequence of their irregular surface, which occasions an unequal pressure of the rubber): and to obviate this inequality still more effectually, the rubber is so formed, that the back of it acts as a horizontal spring; which keeps up a steady, and nearly uniform pressure, without the imperfection of shortening the negative insulation. I have never found this machine vary materially in its action, although I frequently employ it through a whole course of lectures without any fresh application of the amalgam; nor do I find that its power of excitation is at all affected by a moist atmosphere.

Its power increased by the multiplying wheels.

Mr. Cuthbertson conjectured, that my machine owed much of its power to the multiplying wheels; and having noticed, that the cylinder made four revolutions for one of the wheel, requested me to try its action with a simple winch. The pressure remaining the same as in the former experiments, it required (as might have been expected) four times the number of turns to produce any given effect. The friction however was materially less, 2½lbs. troy being sufficient to move the handle from its horizontal position.

These greatly increased the friction.

Applied to the plate machine.

Soon after these experiments Mr. Cuthbertson informed me, that he had discovered an improvement in the plate machine; by which its charging power might be doubled, trebled, or even quadrupled, without any inconvenience but that of increased friction. I could not at first conceive by what means this might be effected; but after some consideration concluded, it could only be accomplished by the application of multiplying wheels to the plate machine; and Mr. Cuthbertson then told me, this was the idea that had occurred to him.

Comparative experiments.

Rubber altered, & new silk applied,

It was agreed, that experiments in proof of Mr. C.'s invention should be made at the conclusion of our comparative experiments; and the requisite apparatus being completed, we resumed our inquiry. Before this second series of comparative experiments, I made some alteration in the rubber of the cylinder, and furnished it with new silk; suspecting, that three years constant wear might have so far

deranged

Increased the power of the cylinder.

deranged it, as to render a comparison with the new plate machine unfair. The result answered my most sanguine expectation. The rubber and its new silk being completed, I found the power of the cylinder machine increased one third; for a trial jar, which had before exploded four times in one turn of the wheel, now afforded six explosions in one turn. The experiment was several times repeated, and the result was uniformly the same.

The following comparative experiments were then made:

Plate machine.

Exp. 1. Length of spark, from the prime conductor of the plate machine, to a two inch ball connected with the ground. The greatest interval, obtained by gradually increasing the distance between the balls, till the limit at which the spark would pass was ascertained, six inches and a half.

Cylinder.

Exp. 2. The same arrangement with the cylinder. The greatest striking distance eight inches and a half. With larger balls, a longer spark could be obtained; I have occasionally procured them upwards of 12 inches; but it is requisite for this purpose to turn the machine very slowly, which occasions undulation.

Lane's electrometer not to be depended on.

Experiments were next made on the charge of a jar fitted up with Lane's electrometer. The following table exhibits the most uniform results obtained in a great number of experiments; and may show how little reliance is to be placed on the accuracy of this test.

Jar of 168 square inches.

Plate Machine.		*Cylinder.*	
Length of spark.	Number of turns required for the discharge.	Length of spark.	Turns of the wheel required for the discharge.
¾ of an inch	2	⅝ of an inch	1¼
1 inch	4	1 inch	2¼
2 inches	8	2 inches	4
6¼ inches	8	5¼ inches	6

Cuthbertson's electrometer.

The anomalies in the experiments with Lane's electrometer are not more remarkable than the uniform accuracy of the following, which were made with Mr. Cuthbertson's most excellent electrometer.

Jar of 168 square inches.

Plate Machine.		*Cylinder.*	
Slider on the arm of electrometer at	Turns of the winch required for the discharge.	Slider on the arm of electrometer at	Turns of the wheel required for the discharge.
15 grains	4	15 grains	3
20 grains	5	20 grains	3½
25 grains	6	25 grains	4
30 grains	7	30 grains	4½
35 grains	8	35 grains	5

No higher charge was employed; for this could not have been obtained without breathing into the jar, which might have occasioned irregular results.

Farther experiments.

A battery of 15 jars exposing 17 square feet of coated surface was next employed, and the following results were obtained.

Plate Machine.		*Cylinder.*	
Slider on the arm of the electrometer at	Turns of the winch required for the discharge.	Slider on the arm of the electrometer at	Turns of the wheel required for the discharge.
10 grains	75	10 grains	63
15 grains	102	15 grains	79

The increased power not exactly proportioned to the velocity of the motion.

The battery was next charged by the cylinder, turned with a simple winch; the slider on the arm of the electrometer remained at 15 grains, the discharge took place in 246 turns. Hence it appears, the advantage gained by the cylinder with its multiplying wheels is not in the exact proportion of the increased number of turns, the intensity being somewhat diminished by the rapidity of the motion: for 70 × 4 = 286, and only 246 were required, a deficiency of nearly ⅛th.

Plate machine.

Multiplying wheels were now applied to the plate machine, the ratio of increase was as follows:

Battery of 15 jars.

Slider on the arm of electrometer at 10 grains

Turned by a simple winch effected the charge in	Turned by multiplying wheels 2 revolutions to 1.	By ditto, 3 revolutions to 1.	By ditto, 4 revolutions to 1.
75 turns.	42 turns.	28 turns.	19 turns.

These

Utility of this improvement.

These experiments sufficiently prove the utility and importance of Mr. Cuthbertson's improvement of the plate machine, by the application of multiplying wheels; for when allowance is made for some defects in the machinery we employed, (which had only been mounted in a rough manner for trial); it will be found, that the charging power increased so nearly in proportion to the number of revolutions; as to exhibit in no instance a deficiency of more than $\frac{1}{8}$th: and in the last experiment with the plate machine, the diminution is only $\frac{1}{76}$th; although the original charging power is quadrupled. Thus in the three experiments in which the motion of the plate was accelerated; 42 × 2 = 84; 28 × 3 = 84; and 19 × 4 = 76. The numbers required by the simple winch was 75, and 75 is to 84 nearly as 7 to 8.

Principle capable of being carried farther,

As there is no reason to suppose, that our experiments have reached the limit, to which this charging power may be increased; it is fair to conclude, that, by the proper application of a moving power, the quantity of electricity given out by any machine in a determinate time may be *doubled*, *trebled*, *quadrupled*, or even increased *six* or *tenfold*. The discovery of this principle is therefore of the highest importance, as it offers the most effectual and ready means of obtaining a very considerable accumulation of the electric fluid; a circumstance of considerable interest in the present state of electrical and chemical inquiry.

and of great importance.

Comparative advantages of the two machines.

With respect to the comparative advantages of the different instruments, much might be said; at present it may suffice to state only those facts, which are of general utility. When we first endeavoured to ascertain the power required to put each machine in motion, it appeared, that the cylinder required 14lbs. and the plate only 8lbs. It was afterward found, that this disparity arose in part from an inequality in the levers, by which they were moved; when these were made equal, the difference was much less considerable, 10lbs. were required for the cylinder, and 8lbs. for the plate; and this difference in their friction will not be found more than commensurate to the difference in their acting power.

The principal advantages in the cylinder are, 1st, the positive and negative powers are obtained in equal perfection; 2d, it has but one rubber to keep in order; 3dly, it is less liable (from the security of its form) to accidental fracture, than the plate; 4thly, its insulation is more perfect; and 5thly, from the peculiarity of its structure, larger multiplying wheels may be employed, and thus a considerable diminution of friction be obtained.

Those of the cylinder.

The advantages of the plate machines are, 1st, they are less expensive than cylinders of equal power; 2dly, they occupy less room; 3dly, may be constructed of a much larger size, as instanced by Mr. Cuthbertson's large machine at Harlem; 4thly, several plates, to act jointly, may be more easily combined, than several cylinders could; 5thly, the multiplying power may be applied to them to a much greater extent, than it could to cylinders, without rendering the motion too rapid; 6thly, plates of equal diameters may be made to act with a uniform and equal degree of power, a circumstance seldom attained by cylinders.

Those of the plate machine.

These are the only conclusions the present state of our inquiry seems to warrant; many experiments remain to be made, and when a sufficient number of these are completed, I shall not delay the communication of them to the public.

Farther experiments to be made.

3, *Princes street, Cavendish square,*
June 13*th*, 1810.

XIII.

Researches on Acetic Acid, and some Acetates: by RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A. &c.*[*]

THE identity of the acids contained in vinegar, and in the product of the distillation of verdigrise, is now generally admitted; and the terms acetous acid and acetites have

No acetous acid.

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[*] Annales de Chimie, vol. LXIX, p. 5.

been finally erased from the list of chemical substances, in which, for more reasons than one, they ought never to have been inserted.

Distillation of metallic acetates.

Though several chemists have already turned their attention to some of the subjects of the present inquiry, I believe I have some new facts to add on the distillation of metallic acetates; and without any intention to diminish the claims, which Messrs. Courtenvaux, Lauraguais, Monnet, Lassonne, Edenzel, Berthollet, Chaptal, Proust, Higgins, Pelletier, Adet, Darracq, Dabit, Trommsdorff, Derosne, &c. have to our acknowledgments, I shall proceed to relate the whole of my researches, begun in March 1803, but which I was unable to finish before the present moment (january the 11th, 1808.)

English vinegar distilled.

Spirit from it.

In a glass retort I carefully distilled two quarts of vinegar, made in England from malt. Its specific gravity had become 1·0042. I saturated it with carbonate of potash, and distilled it again to dryness. What remained in the retort was acetate of potash coloured by vegetable matter. The liquid that came over was perfectly clear and colourless, and retained a slightly spirituous smell, which I had observed in the vinegar before distillation. To separate this spirit from the water with which it was mixed, I threw into the liquid a large quantity of dry carbonate of potash. The water being saturated with this, the surface of the solution was covered with a very thin pellicle of this spirit; too little indeed for me to estimate its quantity, but enough to leave no doubt of its existence.

Mucilage, or extract,

The liquid, which was clear and colourless, as I observed, after distillation, and before I had thrown any carbonate of potash into it, was rendered turbid by the solution of this salt, and became of a violet brown. Some flocks remained suspended in the water. It is this matter which Mr. Darracq calls mucilage, but which, according to Mr. Steinacher, is extractive matter.

passed over twice in distillation.

It is to be observed, that this substance, whether mucilage or extract, had passed over in distillation twice: first when I distilled the original vinegar, and next when I redistilled it after the combination of its acid with potash. I found it very difficult to deprive vinegar of this entirely by repeated distillations.

French vinegar.

In a similar way I examined French vinegar. That I used was of the specific gravity of 1·0072. The proportion of its acidity to that of the English vinegar was as 4·01 to 3·46. In general it contained less mucilage and more spirit than the English. I met with a vinegar in the shops at Paris, that contained a very sensible portion of alcohol. Four quarts distilled from carbonate of potash yielded two ounces of a very light fluid, from which I separated 0·46 of ardent spirit.

Component parts.

From these experiments it appears, that the vinegar I employed was composed at least of water, acetic acid, vevegetable matter, and a small portion of a spirituous liquor.

Acetate of copper distilled.

I distilled 4lbs of acetate of copper, dividing the product into five nearly equal parts. Each of these I rectified by a second distillation to dryness. The first portion had the specific gravity of 1·0659: the third of 1·0580; the fourth of 1·0454; the fifth of 1·0400. An accident prevented my examining the second. On saturating these portions with the same base, I found the quantities of acid contained in them to be in the following proportion; 62·971 for the first; 67·461 for the third; 74·411 for the fourth; and 73·295 for the fifth.

But this series, the last term excepted, is increasing, while that of the specific gravities is uniformly the reverse.

These facts, lately noticed by Messrs. Derosne, observed long ago.

Messrs. Derosne have just published a paper on this subject; but when I observed the same facts in 1803, I found from the scientific collections of the last century, that Courtenvaux, Monnet, and Lassonne had noticed them fifty years ago.

In the year 1754 the Marquis de Courtenvaux, examining the present subject, says, that the first portion that passes over on distilling acetate of copper is not inflammable; and that, though heavier than the subsequent portions, it is less acid. This perfectly agrees with what I have just said. In the same work he observes, that the last portion easily takes fire; and other chemists have made the same remark. The smell of the last portions too is more pungent than that of the first. All these appearances depend on a spirituous very light, highly inflammable, and extremely pungent fluid, which forms at the close of the distillation of acetate of copper. Messrs. Derosne have given it the name of pyroacetic ether; but this seems to me to be determining its nature too precisely, and I have called it by the more general term of pyroacetic spirit.

Their pyroacetic ether more properly spirit.

No experiment I made indicated the presence of mucilage in the liquid product of the distillation of verdigrise. It appears to be composed at least of water, acetic acid, and pyroacetic spirit.

Difference between acetic acid and vinegar,

From these results we may appreciate the slight but real differences, that exist between vinegar and the product of the distillation of acetate of copper: and which had formerly led Berthollet and Chaptal to believe the existence of two acids.

In English vinegar, for instance, there is a little acetic acid, a little vegetable matter, and extremely little spirituous liquor. In the product of distilled acetate of copper there is more acid it is true, but no vegetable matter, and much more spirituous liquor. On account of the lightness of this liquor therefore, there ought to be more acetic acid in the product of the acetate of copper, than in vinegar of equal specific gravity. Accordingly Mr. Berthollet, having saturated equal quantities of these two fluids reduced to the same specific gravity, obtained more acetate from the former than from the latter: and Mr. Chaptal has observed, that vinegar required one sixth less of base than the product distilled from acetate of copper, under similar circumstances. I have composed liquids in which a quantity of acid was compensated by a portion of pyroacetic spirit, in order to keep the specific gravity the same as that of vinegar; and the quantity of base required to saturate each was exactly in the ratio of the spirituous liquor.

Vegetable matter.

The vegetable matter too accounts for vinegar being more highly coloured by the addition of sulphuric acid, than the product of acetate of copper. It accounts for the greater quantity of carbon in the destructive distillation of the acetate formed by the combination of vinegar with potash,

than

Purification of acetate of potash,

than in that of the acetate obtained by the union of this alkali with the product of acetate of copper. This it is that obliges the druggists to expose what they call foliated earth of tartar to a moderate heat, in order to render it white. Lastly, when Mr. Berthollet imagined, that by means of acetic acid he had expelled acetous acid from its combination with potash, it was because we can in fact obtain an acid milder than the acetic, and nearer to the state of vinegar, by distilling acetic acid from an acetate of potash in which all the vegetable matter has not been destroyed by heat. Part of this, I believe, is carried off by the fresh acid, till the equilibrium of affinity between it and the salt in which it existed before is restored.

Spec. grav. no test of acidity.

From these experiments it follows, that the specific gravity is not a faithful index of the quantity of acetic acid contained in vinegar, and in the product of the distillation of acetate of copper, because neither of them is a pure and simple solution of acetic acid in water.

Acetate of lead, or of zinc yields more spirit than that of copper.

To place all this in a more striking light, I availed myself of the researches of Lassonne and Monnet, who have instructed us, that the acetate of lead, or of zinc, distilled like that of copper, gives us a larger quantity of the spirituous liquor. Accordingly I subjected to distillation two pounds of acetate of lead, and collected the produce in three portions. The first was nothing but weak acetic acid; the second, rectified by distillation to dryness, was of the specific gravity of 0·9234; and the third, of 0·8567. Their acidity was in the ratio of 4·421 to 5·462, and the spirituous liquor as 60·50 to 83·25.

Other metallic salts compared.

Comparing the various properties of lead and copper, and particularly their reducibleness, with the difference of the results in the distillation of their acetates, I was led to inquire into the action that takes place between some other metals and vegetable acids in similar circumstances, in hopes of connecting together the phenomena of the distillation of metallic acetates, and deducing from them a general law respecting the formation of the pyroacetic spirit.

Resistance of a salt to heat, as

The resistance, that any salt opposes to the action of heat, is proportional, *cæteris paribus*, to the affinity of the acid the affinity of its parts, unless one of these is destructible.

for its base. But if the acid, or base, be decomposable at a lower temperature than would separate them were it not for this destructibility, the affinity that unites them is a function of that on which their composition depends. The phosphate of lime does not lose its acid at a temperature, at which sulphate of lime parts with its acid, because the latter is decomposed.

Metallic oxides.

Let us take then for bases the metallic oxides; but let us first unite them with one particular acid, so that all the differences in the results of the decomposition of the salts they form may depend on the variation of one substance alone.

Circumstances to be attended to.

It is true, that each base is accompanied with fresh circumstances, independently of the facility with which it is decomposed. The proportions too of water, of base, and of acid, are not to be neglected.

State of the oxide before the process.

Another essential consideration is the state of the oxide before the process, and that to which it has a tendency during its taking place. Metallic silver, for example, is not soluble in any acid: manganese oxided at 0·66 is equally insoluble; but after adding oxigen to the one, and abstracting oxigen from the other, saline combinations of these metals may be formed. Chemists speak of the gray oxide of silver with 10 per cent of oxigen, and the white oxide of manganese with 20 per cent, as those of their respective metals that are most soluble. But this gray oxide of ſilver is reducible by a gentle heat, and the white oxide of manganese is liable to be superoxided by taking up 46 per cent more of oxigen. We must pay attention therefore to the 0·10 that one gives out, and the 0·46 that the other may absorb.

Acetates of silver and manganese.

The acetates formed by these two oxides may be considered, in this point of view, as the extremes of a series, the intermediate terms of which are to be found among the other oxides. Let us then examine in detail the acetates of ſilver, copper, nickel, lead, iron, and manganese.

Composition of the salts.

To compound these salts in a uniform manner, I prepared the oxides of the different metals by the most appropriate means the art of chemistry teaches us; and I always satisfied myself of their purity, before I made use of them. I then combined them directly with distilled vinegar, or with pure acetic acid, according to the object I had in view.

Acetate of silver.

The oxide of silver dissolves very well in vinegar, and in diluted acetic acid; and the salt it forms with both is so remarkable, that it would be sufficient to prove the identity of the two acids. On evaporating the solution, a pearly acicular salt is obtained, grayish if vinegar be employed, white if formed with the pure acid, very light, and very soft to the touch. If 100 parts of the mother water of these crystals at a heat of 15° [59° F.] be evaporated, about one part of salt will remain. If the mother water be hot, more will be obtained. This salt may be procured likewise by pouring a concentrated solution of an alkaline or earthy acetate into a solution of nitrate of silver, washing the precipitate, and crystallizing it afresh.

Acetate of copper.

The acetate of copper is well known. I have sometimes operated on that of the shops; but in my experimental researches I formed it myself from its component parts.

Acetate of nickel.

The acetate of nickel I procured by directly combining acetic acid with the oxide of this metal purified by the new processes. It is sufficiently soluble, and crystallizes well. Its solution is a fine deep green; the crystals are of a somewhat lighter colour.

Acetate of lead.

The acetate of lead is at least as well known as that of copper. But it must be remembered, that it exists in two states; that in which it is found in the shops, and that described by Mr. Thenard. Either may be converted into the other, by adding oxide to the former, and acid to the latter. I believe there is a third state of this salt, intermediate between the two.

Acetate of zinc.

The acetate of zinc is very soluble. It crystallizes confusedly, and liquefies in its water of crystallization, the quantity of which is pretty considerable.

Acetate of iron.

If a solution of acetate of iron be left exposed to the open air, red oxide will separate, retaining some acetic acid. Crystals are obtained from it with great difficulty. Some have spoken of a spontaneous inflammation taking place on evaporating a solution of this salt: but I have never seen it, though I have evaporated acetates of iron at least five or six times. I took no particular care, either to produce or to prevent it.

Acetate of manganese.

The acetate of manganese crystallizes better than the acetate of zinc. Rhomboidal laminæ may be distinguished among its crystals. Its colour resembles the smoky topaz, if it be formed with vinegar; but with acetic acid it is whiter. It contains less water of crystallization than the acetate of zinc, yet it liquefies at a high temperature.

Difficult to have the acetates perfectly free from water.

Whatever precautions I took to obtain these salts in a state of dryness, I cannot venture to say, that I have succeeded. Those that crystallize well, as the acetates of silver, copper, and nickel, may be considered as in a state nearly uniform. But the acetate of zinc retains a very large proportion of water in the mass of its confused crystals; and unless it be reduced to very fine particles, it remains in it, and occasions its liquefaction.

Real acetic acid not known.

I would have endeavoured to analyse these salts, if I had conceived any hope of doing it to my satisfaction: but I am not acquainted with the real acetic acid, and I believe it is unknown in chemistry. Some rule however is necessary, by which we may appreciate the state of the substances, on which I operated. The proportion of its weight that each of them loses at a heat capable of decomposing it entirely may serve us as a guide: and accordingly I exposed a known quantity of each to a high temperature in a platina crucible, weighed before and after the operation.

Action of heat on the acetates of silver,

The acetate of silver exposed to the flame of a candle alone gives out a strong smell of acetic acid, and is reduced almost without any change of appearance, so that it resembles native plume silver, It becomes very white, and does not retain carbon, like the other metallic acetates, unless the contact of air be excluded. In open vessels it loses 3·631 of its weight out of 10·000.

copper,

The total decomposition of the acetate of copper is perfectly accomplished in a glass vessel. Exposed in a capsule on a heated sandbath, it first decrepitated. A light vapour of acetic acid then manifested itself at a very low temperature; and the salt became brown round the sides of the vessel. This colour spread gradually to the middle, and the copper acquired a fine metallic brilliancy throughout the whole mass. A pretty bright light then seemed to flash over its surface, and the metal lost its brilliancy, becoming like tarnished copper. It is at this juncture, that the residuum of the distilled acetate of copper has been considered as a pyrophorus. The maximum shown by Fahrenheit's mercurial thermometer during the process was 417°. This experiment, carefully conducted in a platina crucible, in order to appreciate the loss on 10·000 parts, gave 6·441 dissipated by the fire. Some carbon remained with the metallic copper.

nickel,

The acetate of nickel, exposed to an open fire, lets its acid escape. It becomes black, and we see flashes of light darting over its surface, as in the process with acetate of copper. It retains a little carbon after the operation, and loses 6·261 of 10·000.

lead,

Ten parts of acetate of lead of the shops swelled up before losing their acid. A greater heat is required to decompose this salt, than those I have mentioned; and the smell is not that of acetic acid. It becomes at first black, then yellow, then red, when heated in the open air; and its carbon burns as well as the metal. It loses 3·552. That of Mr. Thenard does not swell up. In other respects it exhibits the same appearances, but loses in the fire only 1·635.

zinc,

The acetate of zinc loses its acid, and becomes less black. It burns, and grows black. It loses 6·025; but as a part is volatilized, this experiment is less to be depended on than those with the other acetates.

iron, and

The red mass of acetate of iron loses commonly about 4·500: but it is difficult to obtain it in a uniform state.

manganese.

The acetate of manganese exhibits similar phenomena, and loses 7·186.

The proportions I shall now proceed to give relate only to the salts that lose in the fire the quantities abovementioned.

Distillation of these acetates.

For the distillation of these salts, I introduced a known quantity of each into an earthen retort, or one of coated glass, according to the degree of heat necessary for the operation. To this I joined a tubulated matrass, and a Woulf's phial containing solution of barytes; and the pneumatochemical trough terminated the apparatus. I began the operation with the lowest degree of heat possible, and always endeavoured to keep it at a minimum.

Results.

The results were three products, which it was necessary to examine. First there remained in the retort the metallic base; and to ascertain the state in which it was left by the acid, I submitted it to the docimastic experiments suited to each metal.

Residuum of the acetates of silver,

The residuum of distilled acetate of silver dissolved in nitric acid with evolution of nitrous gas. A black matter remained, which did not dissolve. This, when washed and dried, weighed 0·05, and was charcoal. The nitric solution produced the same quantity of muriate of silver as 0·95 of metallic silver.

copper,

Messrs. Adet and Darracq have said, that, after the distillation of acetate of copper, the metal is oxided, according to the former with 0·08 of oxigen; and according to the latter so as to be soluble in muriatic acid. Hitherto only two oxides of copper have been mentioned; one, which is brown, containing 0·20 of oxigen; the other red, containing 0·17 of oxigen according to Proust, but from experiments on the octaedral red copper ore from Cornwall I conceive it should be estimated at 0·115. I dissolved in nitric acid 10·000 of the residuum of the distillation of acetate of copper. I filtered the solution, and 0·055 remained on the filter, which had all the properties of carbon. The filtered solution I evaporated to dryness twice, adding muriatic acid each time. I then precipitated the copper by purified zinc, and obtained 9·4 of metallic copper. Now if the copper had not been in the metallic state in the residuum of the distilled acetate, I should not have obtained the original weight of the matter employed; and the deficiency would have indicated the quantity of oxigen. On another 10·000 parts I poured muriatic acid, carefully preventing the contact of air: and at the expiration of ten days not an atom of copper was dissolved. By reduction by fire, by alkalis, and other docimastic means, I satisfied myself, that the copper is truly in the metallic state in the residuum of the distillation of acetate of copper. Mr. Darracq found an insoluble residuum of 0·22 after the action of muriatic acid. In my process I found but 0·05 or 0·06, beyond which there was no sensible variation. His residuum must surely have contained copper, that had escaped the very imperfect action of the muriatic acid.

nickel,

The distilled acetate of nickel leaves a black residuum. In muriatic acid 10·000 parts dissolved with a brisk evolution of hidrogen gas; and 0·14 of carbon remained. The nickel in this residuum was found to be in the metallic state; for it yielded the same quantity of precipitate by alkalis as 10·000 parts of nickel, deducting the carbon.

and lead.

The residuum of the distillation of acetate of lead is in the metallic state. It leaves 0·04 of carbon; and precipitated from its solution in nitric acid by a sulphate, it yields the same quantity of sulphate of lead as an equal weight of metallic lead. Mr. Trommsdorf has said, that what is left in the retort after this distillation is oxide of lead: but I never found the lead in the process oxided but once, and that was when the retort had cracked. It is only when the acetate of lead is exposed to the contact of air during the distillation, that the metal is oxided, as I have already observed in speaking of the loss it undergoes when exposed to heat in open vessels.

All these pyrophori.

The residuum of the distillation of all these acetates, which contain carbon, are more or less pyrophori. It is not at all strange, that charcoal when in a state of minute division should take fire more easily than when compact.

Residuum of the acetates of zinc,

The residuum of distilled acetate of zinc dissolves in muriatic acid directly, and without effervescence; and 0·050 of charcoal remain. The zinc is in the state of white oxide, as it was in the salt previous to distillation.

iron, and

After the distillation of acetate of iron, black oxide of iron remains, with 0·02 of carbon. It dissolves in muriatic acid, and yields the muriate formed by black oxide of iron.

manganese.

The acetate of manganese leaves a brown residuum. When muriatic acid is poured on it, it dissolves, giving out oximuriatic acid, and leaving 0·035 of carbon.

Liquid products of the distillation.

We will now proceed to examine the liquid products. The matrass in which they were condensed was constantly surrounded with a frigorific mixture, as was its neck also, which I always chose very long. In consequence nothing escaped but permanently elastic fluids.

In these liquid products three things were to be ascertained: their specific gravity, their degree of acidity, and their proportion of spirituous liquor.

Method of finding specific gravity.

To find the specific gravity, I first took the weight of a small phial with a ground stopple, which at the temperature of 15° [59° F.] contained exactly 10 gram. [154·4 grs] of distilled water. The weight of any other fluid contained in this phial at the same temperature gave the specific gravity directly. This method, on which Mr. Descroizilles has published a very minute paper, is far from new; but he has not ascribed to it all the advantages it actually possesses. It merits the preference particularly because it avoids all friction from immersion in the liquor to be assayed, and has no limits but the sensibility of the balance employed. That I used was sensible to a thousandth of a gramme, which gave me the specific gravity sought to a ten thousandth nearly.

Objections answered.

To this method it has been objected, that the stopple may be thrust in more or less in different experiments, so as to force out more or less of the fluid. The dilatability and elasticity of glass too have been mentioned. But let any person repeat the experiment several times on the same fluid, andhe will find, that the differences will only affect the thousandth or ten thousandth parts. This bottle of a known weight is equally adapted for taking the specific gravity of solid bodies.

Methods of finding their degree of acidity.

To find the proportion of acidity of these liquids, I had hoped, that by employing a substance, which by its own action on a solvent should be divided into a soluble and insoluble matter, I should be able to substitute a law of nature for the uncertainty of manipulations. Accordingly I tried carbonate of lime. But acetic acid does not attack this with sufficient vehemence, to attain the limit of its action in a uniform manner: and having put some carbonate of lime into a glass retort, in order to dissolve it in acetic acid, I collected the fluid that passed over by distillation, and found it required repeated cohobation, before it ceased to give signs of acidity; which rendered the process complicated, and deprived it of precision.

I had equally hoped, that carbonate of potash thrown into acetic acid would have given me the ratio of acidity by the quantity of carbonic acid evolved: but I found, that trials of this kind differed more from each other than the following.

That adopted.

I exposed potash to a strong red heat, yet I was far from supposing it to be totally deprived of water. For more facility of proceeding, and to reduce any errour to a tenth part, I dissolved one part of this potash in nine parts of distilled water. Into a given quantity of the liquids resulting from the distillation of the acetates I poured some of this solution; and when the tinged papers indicated, that the point of saturation was at hand, by letting it fall drop by drop from a very slender tube I attained a degree of accuracy more than sufficient, to answer the other parts of these researches.

(To be concluded in our next.)

XIV.

On the Precipitation of a Solution of Sulphate of Lime by Sulphuric Acid. By T. LE GAY BREWERTON, *Fellow of the Royal Physical Society, Edinburgh.*

To Mr. NICHOLSON.

SIR,

IN a paper on the acids produced by treating ginger root with nitric acid, which you honoured with a place in your Journal*, was this note.

Sulphate of lime precipitated by sulphuric acid,

"The fact of sulphuric acid causing the deposition of "sulphate of lime from a state of solution, to me is not a "little surprising, however it may perhaps be well known "to those more versed in chemical experiments and in no "manner puzzling."

owing to impurity of the acid,

That sulphate of lime may be rendered more soluble by an excess of acid, is a well established fact; but that a still greater excess of acid should cause the deposition of the sulphate already dissolved, seems an operation of so anomalous a nature, that it cannot be received as true without the greatest scepticism. Impressed with this idea, I attempted to discover the source whence the deception had originated, and found it to be in the sulphuric acid.

The

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* Vol. XXV, p. 177.

which contained sulphate of lime.

The sulphuric acid contained a quantity of sulphate of lime, which was deposited on diluting the acid, and which presented crystals precisely similar to those in the paper alluded to.

Though being deceived by the impurity of the sulphuric acid does not affect the general conclusions as to the acid products obtained by treating ginger root with nitric acid, yet it shows the crystallized matter in the experiments did not arise from using white lead, although it was adulterated with chalk, but from using impure sulphuric acid; and shows too the necessity of young experimentalists paying particular attention to the purity of the reagents employed.

Necessary to examine the purity of reagents.

I am, Sir, yours very respectfully,

Bawtry, June 13, 1810. T. L. G. BREWERTON.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineralogy of the Highlands.

AT the meeting of this Society on the 7th of April, Dr. Macknight read a mineralogical notice, on the tract of the Highlands from Killin to Braemar, by the way of Glen Zilt. Ben Lawers is composed of undulated mica-slate, which at the summit is yellowish-gray, and in some varieties so full of quartz as to resemble a sandstone. Towards Lagierait, beautiful garnets begin to appear. Beyond Mullevearn, gneiss occurs; also limestone, hornblende-slate, and sienite. Besides the substances first mentioned, Glen Zilt is remarkable by a peculiar aggregate of feldspar, hornblende, and occasionally quartz; in which the various proportions of these ingredients exhibit the rock under various aspects of the sienitic and greenstone species. It is distinguished from granite (for which it has been mistaken) not only by the uncrystallized state of the feldspar, but by the presence of hornblende, and the absence of mica. Professor Jameson has called it sienitic greenstone. It occurs in conformable beds; particularly one of great size, which intersects the channel of the river at different places, near the lodge. Crossing the mountains from Glen Zilt to the course of the Dee, we find hornstone, feldspar-porphyry, and limestone, subordinate to mica-slate and gneiss; till we reach the castletown of Braemar, where the granite of the Grampians at length appears.

At

Vertical conglomerate rock of the Grampians.

At the same meeting, a communication from Col. Imrie was read, describing the conglomerate rock of the Grampians, and tracing it from near Stonehaven to the Burn, and again at Callender, eighty miles distant to the N.W. The position of this conglomerate rock is vertical; and of this fact, in Col. Imrie's opinion, no satisfactory explanation has yet been given.—At this meeting, also, there was laid before the Society an accurate section of the coal-field at Alloa, accompanied with interesting remarks, by Mr. Robert Baid, civil engineer, and manager of Mr. Erskine of Mar's extensive coalworks. The depth of the section is 704 feet; the alternating strata are 141 in number; and the total amount of the thickness of the different beds of coal, is 59 feet 4 inches. Captain Laskey likewise presented to the Society a cabinet containing a series of the remains of a fossil encrinus found in slate-clay near Dunbar.

Coal field at Alloa.

Fossil encrinus.

Mathematical Repository.

Mr. Leybourn of the Royal Military College, has just published the tenth number of the Mathematical Repository, containing; solutions to the mathematical questions proposed in the Eighth number, and a series of new questions to be answered in a subsequent number; an Essay on Polygonal numbers; a new demonstration of the Binomial Theorem; an illustration of the forty-seventh proposition of the second book of the Principia; a curious indeterminate problem; solutions to a curious problem in Dynamics; and a continuation of Le Gendre's Memoir on Elliptic Transcendentals.

Treatise on Fluxions and the application to naval and military science.

Mr. W. Moore, of the Royal Academy at Woolwich, has in a good state of forwardness a Treatise on the Doctrine of Fluxions, with its application to all the most useful parts of the true Theory of Gunnery, and other very important matters relating to Military and Naval Science. The fluxions will be preceded by such parts of the science of mechanics, as are necessary for reading the work without referring to other authors; and the whole will be so arranged, that any person moderately skilled in algebra, geometry, and trigonometry, and having a knowledge of the most common properties of the conic sections, may proceed to these inquiries without any difficulty. The whole will be printed in 1 vol. 8vo, and will be particularly adapted to all Military Institutions of eminence,

Meteoro-

METEOROLOGICAL JOURNAL,

For JUNE, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAY Day of	Thermometer.			BAROMETER, 9 A. M.	WEATHER.
	8 A. M.	Noon.	11 P. M.		
28	48°	56°	45°	30·06	Fair
29	46	51	46	30·38	Ditto
30	49	65	51	30·36	Ditto
31	52	66	50	30·32	Ditto
JUNE.					
1	53	65	52	30·29	Fair
2	54	69	54	30·25	Ditto
3	53	66	50	30·24	Ditto
4	52	65	55	30·26	Ditto
5	54	55	50	30·24	Cloudy
6	51	70	55	30·24	Fair
7	54	67	56	30·16	Ditto
8	56	70	61	30·12	Ditto
9	59	73	55	30·00	Ditto
10	59	70	55	29·73	Cloudy
11	58	68	54	29·80	Fair
12	54	66	53	29·95	Cloudy
13	55	60	50	29·90	Rain
14	50	63	54	30·10	Fair
15	54	66	51	30·24	Ditto
16	52	62	46	30·09	Ditto
17	50	60	54	29·98	Ditto
18	56	70	60	29·94	Ditto
19	60	70	60	29·98	Cloudy
20	63	72	63	29·96	Ditto
21	64	77	66	30·18	Fair
22	66	73	55	30·30	Cloudy
23	54	69	55	30·40	Fair
24	56	73	60	30·23	Ditto
25	59	73	66	30·16	Ditto
26	59	61	58	30·09	Cloudy

A
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OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1810.

ARTICLE I.

Analysis of the Galvanic Pile. By J. A. De Luc, Esq. F.R.S.

Part II.

Pile separated by writing paper.

I Concluded the first part of this *Analysis* by an experiment, in which a pile of 76 groups of *zinc* and *silver*, the number used in my preceding experiments, separated by pieces of *writing paper* substituted for the *wet cloth*, though giving very sensible *electric* signs at its extremities, produced no *chemical* effects in the water of the usual glass tubes applied to it, nor the *shock*.

Difference between the electrical & chemical effects of the pile.

This experiment was made during my investigation respecting the causes of the two distinct effects of the *pile*, namely *electric* and *chemical*, which by the foregoing experiments had been proved to be different; the former depending only on the *binary groups* of the *metals*, separated by the best *conducting* substance *nonmetallic*, and thus not necessarily productive of *chemical* effects; these requiring absolutely, that the interposed *nonmetallic* substance be *wet*; a circumstance from which the *chemical* effects depend on

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9	59	73	55	30·00	Ditto
10	59	70	55	29·73	Cloudy
11	58	68	54	29·80	Fair
12	54	66	53	29·95	Cloudy
13	55	60	50	29·90	Rain
14	50	63	54	30·10	Fair
15	54	66	51	30·24	Ditto
16	52	62	46	30·09	Ditto
17	50	60	54	29·98	Ditto
18	56	70	60	29·94	Ditto
19	60	70	60	29·98	Cloudy
20	63	72	63	29·96	Ditto
21	64	77	66	30·18	Fair
22	66	73	55	30·30	Cloudy
23	54	69	55	30·40	Fair
24	56	73	60	30·23	Ditto
25	59	73	66	30·16	Ditto
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Conducting faculty of organized substances owing to moisture.

The *conducting* faculty of all the substances of the *vegetable* and *animal* kinds, that may be employed to separate the *binal groups* of *metals*, for transmitting from one to another their individual *electric* effects, mostly depends on a certain quantity of *moisture,* with which they are usually penetrated. Of this I had soon an opportunity of being informed, by an experiment of my friend Dr. Lind, who had constantly followed the progress of these researches: he had constructed a pile similar to mine, except that *copper* plates were substituted for *silver* plates, which acted also immediately upon the gold-leaf electroscope; but he found the following change in it.

Exp. 21. The paper highly dried. The action diminished.

Exp. 21. He dismounted this pile, and laid all its parts on his hearth, before a great fire, so that the pieces of paper were almost singed, and mounting it again in this state, it did not sensibly affect his electroscope: he brought it into my room, and it affected only the electroscope of my condenser. We then dismounted the pile, and laid all its parts on a table for one hour, my hygrometer being about 40°, and when again mounted, it acted on the electroscope as it did before the *papers* had been so thoroughly *dried* by a great heat.

Water not an insulating body for low intensities.

This experiment shows, that Mr. Davy was mistaken when he thought, "that with respect to *electricities* of such "*low* intensities, *water* is an *insulating* body." On which principle he considered the *mode of action* of the *pile* under the idea of what he calls *induction*; meaning, I suppose, something similar to the effect of Volta's condenser, when it operates by the interposition of *silk* between the *metallic* plates. If this were the case, pieces of *silk,* through which what he calls *induction* is readily produced, being placed between the binary groups of *metals*, should produce a still greater effect than *paper* in its common state, as more *insulating*; whereas the foregoing experiment proves, that the effect almost ceases, when *paper*, by thorough *dryness*, is reduced to the state of an *insulating* body. I insist on this point only, because it involves one of the most important questions in terrestrial physics. Must we, *in the present state of our knowledge*, be satisfied with *electrical energies*, which might be considered as *essential properties of matter**? Or rather, in this very *state of our knowledge*, is it not already ascertained, that a particular *substance* exists, namely, the *electric fluid*, which, beside the effects here in view, produces greater and more general effects on our globe? This question is the main object of this and the following papers, and I hope it will be decided by facts.

Electricity not a quality of matter,

but the effect of a distinct fluid.

Having found no *conducting* substance more convenient than *paper*, for transmitting from one to another of the binary groups of *metals* their individual effects, I came to consider whether it might not be of some advantage, on account of the very small quantity of *electric fluid* thus set in motion, to produce a closer contact of the *paper* with the *metals* between their groups. I made various experiments, by pasting the *paper*, first on both *metals*, then on one only, in the intervals where I had placed it loose. My condenser rendered easy these trials, as I could make them on piles of only 20 groups, and the following was the general result. There is an increase in the *transmission* of the *electric fluid*, when the *paper* is pasted on the outside of such group, upon the *metal*, which becomes *negative*; but the reverse takes place, when it is pasted upon the *metal* which becomes *positive*.

The transmission of the electric fluid promoted by pasting the paper to the negative metal, & *vice versa*.

In the following experiments each pile of 20 groups was placed upon the moving metallic pillar of my condenser, and raised into contact with the receiving plate of the latter, where I left it during 20 seconds; then, letting it down, and removing the upper plate, I observed the divergence of the gold leaves, and noted it in *decimals* of an inch, as in the experiments of the 1st *part*. In these operations, the end of the small piles, which rested on the pillar communicating with the ground, was thereby *neutral*; and the whole *electric difference* between its extremities was expressed, with its proper *sign*, by the electroscope of the conden-

Mode of conducting the experiments to prove this.

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* Mr. Davy's *Bakerian Lecture*, Ph. Trans. 1807, Part I, p. 39; or Journal, vol. XIX, p. 50.

ser. The first experiment on each association of *metals* was made with the papers *loose* between the *groups*, in order to observe afterward the differences resulting from the *pasting* them on one or the other of the *metals*.

Exp. 22. *Exp.* 22. With 20 groups *zinc* and *silver*, separated by *loose* pieces of *paper*.

Silver side on the *pillar*, *zinc* side applied to the *cond.* *pos.* 0·35.

Zinc side ············ *silver* side ·········· *neg.* 0·35.

The *difference* between the *extremities* being commonly equal, with only opposite *signs*, when these *extremities* are in turn applied to the condenser, I shall suppress the *sign* when the *quantity* only of the *difference* will be the object of consideration.

Exp. 23. *Exp.* 23. This will give the first proof of the advantage of pasting the *paper* on the *metal* which becomes *negative* in contact with the other; this being the case of *silver* in the above pile.

20 groups *zinc* and *silver* separated by *loose* paper····0·35
20 groups············ paper *pasted* on the *silver*··*the gold leaf struck the side.*

Zinc and *silver* (or *copper*) are, as yet, the *metals* which, being associated, differ the most in their *electric* states, and for this reason they are commonly used in the pile; but in order to ascertain whether the advantage manifested by the above experiment, of pasting the *paper* on the *metal* which becomes *negative* in each group, was general, or only a circumstance attached to *silver* and zinc, I thought of employing some *metal*, which became *negative* with *zinc*, but *positive* with *silver*; I found *pewter* such an intermediate metal, from the following experiment.

Exp. 24. Pewter neg. with zinc, pos. with silver. *Exp.* 24. With two *piles*, each of 20 groups, the first *zinc* and *pewter*, the second *pewter* and *silver*; the groups separated by *loose* pieces of *paper*; each *pile* being alternately placed in communication with the *ground* by one side, while the other communicated with the *condenser*.

1st pile

1st pile	*zinc* side applied to the condenser	*pos.*	0·10
	pewter side	*neg.*	0·10
2d pile	*pewter* side applied to the condenser	*pos.*	0·20
	silver side	*neg.*	0·25

These qualities not inherent in certain metals.

This experiment first affords a new proof, that *negative* and *positive* are no *qualities* attached to certain *metals*, since here *pewter* is *negative* in its connexion with *zinc*, and *positive* when connected with *silver*. It is only an accidental circumstance, that there is as yet no known substance with which *zinc* becomes *negative*, and none with which *silver* (or *copper*) becomes *positive*; and they have in themselves no tendency to either of these states, acquiring them, and that inversely, by particular circumstances, as will be seen hereafter.

With respect to my present purpose, this experiment having shown me in *pewter* a metal proper for the trials concerning the pasting of the *papers*, I made the following experiments.

Exp. 25

Exp. 25. With 3 piles, each of 20 groups *zinc* and *pewter*.

1. The *paper pasted* on *zinc* .... almost no effect.
2. .... *loose* between the *groups* .... 0·1
3. .... *pasted* on *pewter* .... 0·5

Exp. 26. With 3 piles, each of 20 groups *pewter* and *silver*.

1. The paper *pasted* on *pewter* .... 0·05
2. .... *loose* between the *groups* .... 0·2
3. .... *pasted* on *silver* .... 0·4

shows, that the conducting substance should be fixed to the negative metal.

Though there is not so much increase of effect in the last, as in the two preceding experiments, it shows still the same general circumstance, that there is some advantage, for increasing the transmission of the individual effects of each *group of metal* to the extremities of the *pile*, in fixing the *nonmetallic conducting substance*, on the outside of each *group*, upon the *metal* which becomes *negative* in its connexion with the other; a circumstance which led me soon to farther steps: for these experiments presented me first the prospect of a *spontaneous*, as well as lasting *electric machine*,

Hint of a spontaneous electric machine.

the

was a gradual diminution in the motion of the *electroſcopes*, and a change in the effects on the pile from the communication of its extremities with the ground.

Unconnected with heat, moisture, or density of the atmosphere.

8. Lastly. These changes could not be attributed to those of either *heat* or *moisture*, at least in my room, and as influencing directly the pile; for the room remained nearly at an equal temperature, and there were but small changes in my hygrometer. I also frequently observed the barometer, and found no connexion between its variations and these great changes in the *strikings* of the gold leaves.

Owing to electricity.

From the whole together of these observations, I could attribute the variations in the *electric* phenomena of the pile only to changes in the *electric* state of the *ambient air*; or those in the effects of the communication with the *ground*, to changeable differences between the *electric state* of the latter and that of the *air*; an object on which we owe to Sig. Volta a great and fundamental truth. This justly celebrated experimental philosopher has made it evident, that *air* possesses the *electric fluid* as well as all the bodies which it surrounds; and on this principle he has founded the only real explanation of the *electric motions* of free bodies, such as the pairs of balls and the gold leaves of our electroscopes. The *electric fluid* tends to an *equilibrium* among all bodies, including the *air*, and there is a mutual attraction between it and all of them, weak with *conductors*, but strong with *nonconductors*, among which is *air*. The proportional quantity of *electric fluid*, which is *actually* possessed by *air*, is the *standard* of *plus* and *minus* in the *electroſcopes*; and the proportional quantity of this *fluid*, which is *actually* possessed by the *ground*, is the *standard* of *plus* and *minus* concerning the differences in the *electric state* of insulated bodies compared with its own: these *standards* are sometimes similar, but they are often different. I shall not enlarge here on this beautiful theory, which I have fully explained in my works, and demonstrated by direct experiments*; but applying it to the pile, the immediate effects of the *ambient air* must be, to give some *electric fluid* to its *negative* side, which has proportionally *less* than itself, and to take some from the *positive*

Volta showed, that the air contains the electric fluid.

Standards of plus & minus.

Effect on the pile.

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* *Idées sur la Météorologie*, published in London by Elmsley in 1786 and 1787.

positive side, which has *more*; and as this influence must change according to the changes in the *electric state* of the *air*, the modifications of the *electroscopes* at the extremities of the pile may lead to unfold the latter, but not without farther discoveries.

Difficulties.

I was stopped in the progress of these researches by an incident, which has occasioned me much labour, and still retards them. This new *pile* is certainly in itself a *meteorological* instrument of great importance, as may be already judged, and shall be further explained in the following paper; but in the state above described it was not fit for regular observations, and till the present moment * I have not yet surmounted all the difficulties: I foresaw them, and it was the reason why I wished, that the beginning of this new career should be soon known to experimental philosophers through the *Phil. Transactions*; in order that it might be followed by others, and probably with more success than by myself; but I am reduced to give the history of my own progress.

The gold leaves at last stuck.

From what has been above explained, the *strikings* of the *gold leaves* in the electroscopes were become the object of observation: for this purpose therefore they ought to have regularly continued; however, after one of the *gold leaves*, at either side, had alternately *struck* and *fallen* for some time, it at last *stuck* to the tin foil. The side therefore, to which this happened, was placed in permanent communication with the *ground*, which made the *strikings* to begin at the opposite side; but there also the gold leaf *stuck*. These adhesions continued, till, by a stroke on the top of the electroscope, the gold leaf fell; the oscillations were then renewed, but again stopped in the same manner. I tried various methods for preventing this adhesion, especially by substituting for the tin foil, which has a rough surface, polished laminæ of many sorts and forms, keeping them even at some distance from the glass, which might contribute to this effect; but all was to no purpose, still the gold leaf would *stick*. This was a great disappointment, and the only method I could devise was, to increase the power of a

Attempts to prevent this.

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* Two years have elapsed since I communicated to the Royal Society the Experiments and Observations which are here my object.

pile, till it could move small metallic balls, in hope that these would not *stick*.

Increase of the power of the pile.

Two ways presented themselves to my mind for this increase of power in a pile; one by uſing *plates* of a larger *size*, the other by increasing the *number* of the *groups*. The first method would have been cumbersome: but for a reason which I shall explain, I did not expect, that it would have the desired effect. However, even for the verification of my conjecture, I made the following experiment.

Exp. 29. Plates of different shape and size.

Exp. 29. I procured 10 *tinned iron* plates 4 inches square; 10 others round, of only 0·5 inch diameter: and I took 10 of my plates of 1·6 inch diameter. I cut pieces of *Dutch gilt paper*, the size of each of them, and compared their respective effects on my condenser. The result was beyond my expectation; I had only conjectured, that the increase of *size* would not increase the *divergence* of the gold leaves; now this was not only verified, but the largest plates produced the smallest effect. However, this unexpected difference probably proceeded from some accidental cause, which I had no time to investigate, and I considered this experiment, which I have often repeated under various forms, only as ascertaining the following proposition: that for the *electric motions*, considered solely with respect to the quantity of *divergence* in the *electroscope*, the *size* of the *plates* is indifferent; though, for the *frequency* of the *strikings* of the little pendula, and the intensity of the effects when the extremities of the pile are connected together, with the same *number* of *groups*, these effects increase with the *size* of the *plates*. This distinction, to which I shall return in the following paper, constitutes a part of the theory, which, as it made me foresee the result of the above experiment, I shall now explain.

Size indifferent with respect to divergence; but the frequency of the strikings increase with the size.

Volta's condenser does not show slight deviations of electricity in small bodies.

I am indebted for the ground of this theory to Sig. Volta; who, when in 1782 he showed me, at Paris, his then new-invented admirable *condenser*, explained to me, that it could not serve to manifest minute degrees of *deviation* from the *electric standard*, when belonging to *small* bodies but only to bodies of such an *extent* or *nature*, that the application of the *condenser* (by this taking its share of that *deviation*) does not sensibly lessen it. As an example of the first case, he gave me the atmosphere; and with respect to the *nature* of bodies, he took a *Leyden vial* discharged without a continued contact, the *residuum* of which, from its *nature*, may affect a moderate sized *condenser* without being much lessened: and to show me the necessity of this condition, he made use of the following analogy. When a piece of ground, by being swampy, indicates some stagnant water, if a *well* be dug there, the water will not fill it up to the level of the *stratum* of earth whence it proceeds, unless this *stratum* be of such an *extent*, that the quantity of water which gathers in the *well* has no sensible proportion with that contained in the stratum; so that the subtraction of this quantity cannot affect the *level* at which the water stands in the *stratum*. We have also an example of this case in the subject of electricity: when an insulated *electrified* body is small, we cannot know its real degree of electrification by applying a common *electroscope;* because this, sharing the *deviation* of the state of the body from the *electric standard*, lessens it too much for expressing what it was before that application.

Explained by analogy,

and by the common electroscope.

I shall use the former of these examples in explaining my theory concerning the difference of effects of the *size* of the *plates*, and the *number* of the *groups*, according to the use of the *pile;* and this explanation will chiefly consist in fixing the points of *analogy* between the two objects.

Theory explained.

1. I compare the *number* of *groups* in the *pile* to the *elevation* of a *stratum* whence water issues into a *well*.
2. The *size* of the *plates*, to the *extent* of this *stratum*.
3. The *degree* of *divergence* in the *electroscopes* at the extremities of the *pile* to the *level* which the *water* can attain in the *well* without *overflowing*.

These first *analogies* are sufficient to explain the case of the last experiment. When we attend to what is directly expressed by our *electroscopes*, we certainly do not expect, that this instrument shall indicate the *quantity* of *electric fluid* possessed by the bodies to which it is applied; for this would require also to measure their surface; we expect only to know the comparative *density* of the *electric fluid* among bodies, or its power to produce certain degrees of

Last experiment explained.

divergence in the *electroscope* we use*. Such therefore are the indications of the *electroscopes* at the extremities of the pile: they express certain *degrees* of *density* of the *electric fluid* on them, which are the same whatever be the *area* of the plates; these *degrees* depending only on the *number* of the *groups*, because each *group* contributes to increase the *density* of the fluid on one of the extremities by lessening it on the other. Thus it is that the *divergence*, both in *plus* and *minus*, of the electroscopes, at the extremities of the pile, is proportional only to the *number* of *groups*; in the same manner that the *height* at which *water* stands

Reasons for saying electroscope.

Hygroscopes and hygrometers.

* I shall take this opportunity of explaining why I use the word *electroscope*, and not that of *electrometer*; it is because there is no instrument entitled to the latter denomination, at least admitted among experimental philosophers. Indeed, of our instruments serving to measure the degrees of intensity of physical causes, I know none *absolute*, except *hygrometers*, such as have been constructed by Mr. de Saussure and myself; for though these instruments are made of different substances, and differ in some other respects, we have obtained in both an absolute *zero*, and absolute maxima in determined cases, as well as determined degrees of intensity, of their object, namely *moisture*: all the other instruments intended for the same purpose, to my knowledge, are only *hygroscopes*, indicating *variations* in *moisture*, without determined *points*, or *degrees* common to them. Thus no kind of *physical instrument* has yet obtained the conditions of an absolute measure, but the above hygrometers, so little thought of by experimental philosophers, though very important in meteorology. The *thermometer* has obtained two *fixed degrees* of *heat*, and determined *divisions* of the interval between them, by which means experimental philosophers understand one another when they indicate certain *degrees* of *heat*. I have also constructed an *electrometer*, which possesses the same conditions with respect to *degrees* of *electrification*, which is described in my work, *Idées sur la Météorologie*; but not having been attended to by experimental philosophers, I have not been induced to follow the extension of this *measure* down to the minute degrees of intensity indicated by the gold leaf *electroscope*, as I could not expect that it should be more noticed: therefore admirable as is this instrument for its sensibility, it affords us no *comparable measure*. In this imperfect state, however, there is, in every *electroscope*, a property which belongs to no other *physical measure*, namely, a *natural* and *absolute* standard of *plus* and *minus*, which is *constant*, as to its general determination, and is the *actual* electric state of the *ambient air*, or the *ground*; though *variable* as to the absolute quantity, as are these electric states; which difference will be one of the objects of this paper.

in a *well* is proportional only to the *elevation* of the *stratum* whence it proceeds.

Theory explained.

4. But when, in order to produce a *current* of water, a pipe is placed, or a trench is cut, on the side of a well, below the *level* at which the water stood in it, the *current* will be greater and more permanent, in proportion to the *extent* of the *stratum*, of the same *elevation*, whence the water proceeds; and also, when the *divergence* of the little *pendula* of the electroscope exceeds the extent that it can have without one of them *striking* the side, then *falling* by a momentary contact with the tin foil, which communicates with the ground, it will *sooner* rise and *strike* again, with the same *number* of *groups*, in proportion to the *size* of the *plates*; which last circumstance increases also the *current* of *electric fluid* circulating in a *pile*, the extremities of which are connected together by a conductor.

5. The *water* of all *springs* has the same source, namely, the *rain water* percolating through the ground, and retained on some impervious *stratum*, either argillaceous or stony. If this *water* do not find in its way any substance, with which it can combine, it comes out as it had fallen on the ground: but if in its course it combines with any substance, it may come out with certain *chemical* properties, different according to the substances which have combined with it. The case is the same with respect to the *electric fluid*, which pervades the *pile*; its source is no other than the *electric fluid* diffused over all terrestrial bodies, therefore over the *pile* itself. However we should be ignorant of the constant existence of this *fluid* over us and around us, were it not that, by artificial or natural operations, its *density* may be either increased or diminished on *insulated* bodies: this is the only circumstance which makes it appear, and that by the *electroscope* alone; for as long as this *fluid* remains in a state of equal diffusion over all bodies, it is manifested by no effect hitherto discovered. The *friction* between two bodies disturbs that *equilibrium*, in a manner which I shall show in a future paper on the *Analysis of the Electric machine*. But in the *pile*, which is my present object, it is by a property of its composition, that the *equilibrium* of the *electric fluid* is disturbed, whence proceed either the motions of the *electroscopes*,

scopes, or a *circulation* of the fluid through the *pile*, when the extremities of the latter are connected together by a *conductor*. Now, in the last of these cases, if the *electric fluid*, in its course, meets with no substance that changes its state; as is the case in a pile composed of *tinned iron*, or *zinc* plates, separated by *Dutch gilt paper*; we are indeed informed by the *electroscopes* of its *accumulation* on one extremity of the pile, and its deficiency on the other; however, neither *chemical* effects in the *circuit*, nor the *shock*, are produced; because the *fluid* remains unaltered: but when it pervades a *pile*, wherein, by a *liquid* being placed between the two *metals*, there is *calcination* of one or both of the latter, new effects appear: if the *liquid* be *pure water*, *chemical effects* are produced in the *circuit*, but there is no *shock*; if it be an *acid*, both effects are produced.

This theory laid before the Roy. Soc. in May, 1808.

These experiments, especially on the different effects of the *number* of the *groups*, and of the *size* of the *plates*, with the above theory on the cause of their different effects, were contained in my paper delivered to the Royal Society the 30th of May, 1808, about one month before Mr. J. G. Children executed in presence of Mr. Davy and Mr. Allen the grand experiment of the same kind related in *part* I of the *Ph. Trans.* for 1809*, by which the theory which I had already announced, was confirmed.

Questions on the mode of action of the pile.

But here two questions arise, which go deeper into the *mode of action* of the *Galvanic pile*, and they are these: 1. Of what *nature* is the *modification* produced in the *electric fluid*, when it pervades a *pile* wherein the *calcination* of some *metal* is going on? 2. What is the *cause* of the *motion* of this *fluid* in the pile, whether producing, or not producing the *shock* and *chemical* effects in the *circuit*?

The nature of the electric fluid answers the first.

The solution of the former of these questions, which leads to that of the latter, depends on the *nature* of the *electric fluid*; a subject much too long to be treated here; but it is fully detailed in both the works I have already referred to†; I shall therefore here confine myself to the conclusions contained in these works, as deduced from uninterrupted series of experiments, of which I shall only describe the part necessary to my subject.

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* See Journal, vol, XXIV, p. 150.

† *Idées sur la Météorologie*, and *Traité élémentaire sur le Fluide électrogalvanique.*

None of its phenomena really explained before Volta.

None of the phenomena observed in our common electrical experiments, namely, the *charge* and *discharge* of the *Leyden vial*, the *electric motions*, the effects of the *electrophorus* and of the *condenser*, had been really explained, till the inventor of the last two instruments, sig. Volta, had formed his theory on the *electric influences*, which threw the first true light on the modifications of the *electric fluid*; and which, in the course of various experiments I made to follow it through all the electric phenomena, gave rise to the system on the *nature* of the *electric fluid*, which I shall here briefly state.

Nature of the electric fluid.

This *fluid*, far from being a *simple* substance, is an astonishing *compound*: and first, in its state which may be called *natural*, that, I mean, in which it is diffused over all bodies, it is found composed of two main parts, from which all the above mentioned phænomena arise. One of these two constituent ingredients of the *electric fluid* in this state is a substance, which, by itself, is not *expansible* (as in *steam*, also an *expansible fluid*, there is a substance which is not *expansible* by itself, namely *water*); this substance in the *electric fluid* I have called *electric matter*; and its function, which I shall soon point out, is very distinct. The other ingredient is an excessively subtile *fluid*, which (as *fire* in *steam*) uniting with the *nonexpansible* substance, produces the *expansibility* of the aggregate. In my French works I have called the latter *fluide déférent*; but here I shall call it *vector*, a short word of the same import, signifying that it *carries along* the *electric matter* (as, in steam, *fire* is the *vector* of *water*).

The electric *vector* instantly pervades all bodies, and carries the *electric matter* through *conductors*, but not through *nonconductors*, such as *glass* and *resinous substances*: when a current of *electric fluid* arrives on one side of a *lamina* of these substances, and its *vector*, in order to establish its own *equilibrium* beyond it, pervades the *lamina*, it deposites the *electric matter* on the surface of the latter, where it remains adherent, till a current of *vector* pervades the *lamina* in the opposite direction, or it is taken up slowly by the *vector* in the air (as *fire* in *steam*, when it pervades a glass *laminâ* to establish its own *equilibrium* beyond it, deposits the *water* on the side which receives the *steam*, where it remains, till it is carried away, either by fire coming from without, or by that spread in the *air*).

I come to the peculiar function of the *electric matter* in the above indicated phenomena: it is the sole cause of *electric motions*, resulting from a *greater* or *less* proportional quantity of it, than is possessed by the *ambient air*; to which subject I shall return: the *vector* has no share in these *motions*, but as the *vehicle* of the *electric matter* acting in their phenomena. (As, with respect to *steam*, it is only *water* that produces the *hygroscopic* phenomena, without any interference of *fire*, except as the vehicle of *water*.)

By this system of a first *composition* of the *electric fluid*, the phenomena, which I have introduced in the beginning, are clearly explained in all their modifications, as I have abundantly proved by direct experiments in my works. But as long as the *electric fluid* remains in what I have called its *natural* state, moving along *conductors* and fixed on *nonconductors*, it produces no *chemical* effect hitherto known: What then does happen, when it produces these phenomena?

The electric fluid in its natural state produces no chemical effects.

How these effects are produced.

If we attend to this change, we shall observe a circumstance *sine qua non*, which is to contain some *cause*; it is, that the *conductor*, along which the *electric fluid* moves, *must* be *interrupted*. Now, when in this case the *electric fluid* darts through the *air*, three new phenomena are observed, *lucidity*, *heat*, and a particular *odour*. This cannot but indicate the *decomposition* of some particles of the fluid, occasioned by an excess of density, from which *light*, *fire*, and an *odorate* substance are disengaged: as when *steam* (to which from the beginning I have compared by analogy this system on the nature of the *electric fluid*) becomes too *dense* for the actual *temperature*, some of its particles, being *decomposed*, emit *water* and *fire*.

These new substances, *light*, *fire*, and an *odorate* substance, thus manifested in the composition of the *electric fluid*, are neither the *electric matter*, nor the *vector*, themselves, but must be contained in them, combined with some other substances, which prevent them from exercising their characteristic effects; a case most common in chemical compounds. The characteristic effect of *fire* is *heat*; when *free* it acts upon the *thermometer*; but it does not, when combined with other substances. *Lucidity* is the characteristic effect of *light*; but this is not *lucid* in phosphori, till they are *decomposing:* and also various bodies, while *decomposing*, emit *odorate* substances, which in their compound state had no *odour*. Now, the *light* emitted by the *electric fluid* probably belongs to the *vector*, which has many properties of the former; but it is not *lucid*, therefore *light* must be combined in it with some other ingredient. The *odorate* substance appears to belong to the *electric matter*, but this has no *odour*, therefore the former must also be combined in it with some other substance. Lastly, the *fire* emitted cannot be referred directly to either the *vector* or the *electric matter;* but probably, during their common *decomposition*, it is itself composed of the *light* and *igneous matter* disengaged. That *fire* is a *compound*, is a system which I have also treated with many experimental details in the above mentioned works.

Fire a compound.

No natural philosopher, who has applied to the study of any main branch of terrestrial phenomena according to the rules of analysis instituted by the immortal Bacon, will be repulsed by the idea of so many *elements* entering into the composition of the *electric fluid*, though hitherto almost excluded from the catalogue of *chemical substances* by a class of chemists, who confine their observations within their laboratories. When, with the view of ascending from some of the most common phenomena to general causes, we have followed this scrupulous analysis by a certain number of regular steps, we are yet, in almost every branch, stopped for want of intelligible *links*, though in series of phenomena manifestly connected together by some common cause; as are for instance many phenomena manifested in our chemical operations, with some which we daily observe in the atmosphere, that great laboratory of nature on our globe. The filling up of these chasms by gratuitous hypotheses is only protracting the attainment of real knowledge.

Nothing to stagger us in this compound nature of the electric fluid.

Let not therefore natural philosophers lose sight of an

This expansible fluid produces various phenomena.

expansible fluid, constantly associated with all terrestrial bodies, and with the *air* that surrounds them; thus present in all our chemical processes, during which some of its *ingredients*, either *engaged* or *disengaged*, might account really for certain phenomena hitherto explained by mere words. For, according to meteorological observations which I shall relate in the following paper, it is by its *decompositions*, alternating with *compositions*, that the *electric fluid* operates in terrestrial phenomena. What were chemical theories before the *chemical* combinations of *fire* with other substances were discovered and attended to! However, as long as this *fluid* shall be considered under the vague idea expressed by the modern word *caloric*, it will not much forward the science of chemistry.

Transmission of electricity through interrupted conductors.

After these general remarks, I return to my subject, which will serve as an example of their application. When the transmission of the *electric fluid* through *interrupted* conductors takes place in a *liquid*, the new phenomena of *lucidity*, *heat*, and *odour*, are not perceived; but there cannot be any doubt, that the *chemical* effects produced in the *circuit*, and the *shock*, proceed from the same *decomposition* of particles, that takes place at *interrupted* conductors, which is visible only through the *air*; for no *chemical* effect is produced in the *water* of the glass tubes, when the *metallic wire* passes through it *uninterrupted*. With respect to the *shock*, this condition is not immediately perceived in the discharge of the *Leyden vial*, because it is sudden, attended with a strong commotion, and not repeated till the *vial* is again *charged*: but with the *pile*, which soon renews, spontaneously, the cause of the *shock*, it has been seen in Exp. 8, that this phenomenon is produced only at the *approaching* contact, and thus by an *interruption*; since all sensation ceased, when I *fixed* the silver spoons on both extremities.

The first question answered.

These preliminary deductions of facts were necessary for the solution of the first of the questions above stated, namely: "Of what *nature* is the modification produced in "the *electric fluid*, when it pervades a *pile* wherein the *cal-* "*cination* of some *metal* is going on?" a question intimately connected with this: "How does it happen, that, "with such a minute quantity of *electric fluid* set in motion by the *pile*, the *shock* and *chemical* effects are produced, while they require a very great quantity of the same *fluid*, when set in motion by any of the other known electric apparatuses?" Being arrived at the general fact above stated, that these effects are never produced but by the *decomposition* of some particles of the *electric fluid*, occasioned by an excess of density, in darting from one point of a *conductor* to another, the answer to the connected questions is obvious: the *modification* undergone by the *electric fluid* in pervading this *pile* is such, that some of its particles are *decomposed* by a very small increase of *density*, when a *conductor* is *interrupted*. We have an analogy of the general case of more easy *decomposition* of *compounds* by previous modifications of the latter, in the processes of *smelting ores*, for obtaining *metals* or *reguli* from them; for an easy separation of the ingredient of the latter must be prepared, by *substraction* or *addition* of other ingredients, and often by both. And as we see, that the *calcination* by an *acid* is necessary to produce the *shock*, it is probable, that the modification of the *electric fluid* in this case is the addition of some *element*.

For the shock some element added to the electric fluid.

2d question.

I come now to the second of the above questions: "What is the *cause* of a *motion* of the *electric fluid* in the *pile?* either producing, or not producing, the *shock* and *chemical* effects in the *circuit?*" The first point to be considered with respect to this question concerns the *nature* of the modifications reciprocally produced by *zinc* and *copper* upon each other, when brought into contact. It is generally said, that, in this case, ZINC becomes *positive*, and COPPER *negative*. But these expressions, according to what has been stated above, cannot relate to the *expansive power* of the *electric fluid*; for, between two bodies in mutual contact, such a *power* must be in *equilibrium;* therefore these expressions must relate to *density*, such as I have defined it. Let us now attend to the experiments.

Contacts of zinc & copper.

These trials are usually made with disks of *zinc* and *copper* (or brass) having an insulating handle in their centre, like the plate of an *electrophorus*, and with the help of the *condenser*; thus, one of the disks being held on the hand, the other is first laid upon it, then brought, by its insulating handle, into contact with the *condenser*. It is commonly supposed, that the latter must be *touched* while it lies on the other; but this, assimilating the process to that of the *electrophorus*, changes the nature of the phenomenon, and may lead into errour. I shall therefore first relate the experiments which I have made, with disks of the two metals about 4 inches diameter, from which the difference between these effects will be seen. But I must premise, that in these experiments, the results are very various, at different times, with respect to the *quantity* of effect; I shall therefore relate first such experiments as I set down in a certain part of one day; then mention the differences.

Exp. 30. *Exp.* 30. I held the *copper* plate on my hand, and laid upon it the *zinc* plate, which I *touched* with my finger in this situation, and then carried to the *condenser*: after 20 repetitions of these alternate contacts, the divergence, then *positive*, of the gold leaves of the latter, was about half an inch.

Exp. 31. *Exp.* 31. I made the same experiment without *touching* the *zinc* plate: the gold leaves struck the sides. I repeated the same process with only 10 alternate contacts, and the same quantity of effect was produced as with 20, when *touching* the *zinc* plate.

I must now mention, that at other times I have found no difference of effect from either *touching* or not *touching* the *zinc* disk while lying on the *copper* disk; and that at different times the quantity of effect in both operations was smaller; a point to which I shall return: but these first experiments prove directly, that when *zinc* is in communication with *copper*, the former takes from the latter some *electric fluid*, making other bodies, on its opposite side, share its *excess*. Thus, after a certain number of repetitions of the alternate contacts, when the upper plate of the condenser is removed, the *fluid* accumulated on the *receiving* plate is manifested by the electroscope: and instead of having increased the effect by *touching* the *zinc* on the *copper*, the finger has sometimes, even during that contact, taken off a part of the accumulated *fluid*. I come to the reverse experiment, made also at a certain part of one day.

Exp. 32. *Exp.* 32. I took the *zinc* disk on my hand, and, placing the *copper* disk on it, I *touched* the latter before carrying it to the *condenser*, which it made *negative*. I repeated the experiment without *touching* the *copper* disk lying on the *zinc* disk, and the *negative* effect was of the same quantity. In general, a greater number of alternate contacts is required to produce the same quantity of *negative* effect with *copper*, than of *positive* with *zinc*.

We see however in this experiment the same phenomenon as in the preceding, in this respect, that when *zinc* and *copper* are in mutual contact, the former takes some *electric fluid* from the latter, which, on its opposite side, it shares with other bodies: in the first experiment it shared this *fluid* with the *condenser*, while the *ground* restored it to the *copper* disk; in the last, *zinc*, communicating that acquisition to the *ground*, took more *fluid* from the *copper* disk, which made the *condenser* partake of its *loss*. This is the leading thread with respect to the *motion* of the *electric fluid* in the *pile*, and I shall follow it; but I must first speak more particularly of the anomalies observed in these experiments, which are important.

Anomalies in these experiments.

These differences, sometimes very great, in the quantity of effect of the same operations, surprised me at first; and suspecting something amiss in the *condenser*, I examined it closely, without finding any defect: then at other times, without any change, I found the same effects. At last I remarked that, commonly, the greatest effects in the same day were in a part of the morning, and the smallest towards the evening; and that these effects differed also in intensity on different days. Now this is what I have said above of the spontaneous effects of the *pile*, which is composed of a succession of the same *binary* groups of *metals*; and thus the *condenser*, when applied to these experiments, is also a *meteorological* instrument. And there is a remarkable circumstance in this respect; that often at the same moment there is a great difference between the effects on the condenser of the opposite extremities of a small pile; but sometimes it is the *negative* side which prevails, and at other times it is the *positive*. This manifests, that the *ground*, with which the opposite side of the small pile communicates, in the former case possesses *less*, and in the latter *more* of the *electric fluid* than the *ambient air*. But this will be one of the subjects of the following paper.

Propagation of the effects through the pile.

After having shown distinctly what are the *electric* effects produced by the conjunction of *zinc* and *copper*, and in what manner the condenser manifests these effects, I come to their propagation from each group alông the *pile*, on one side *negative* and on the other *positive*. The following experiments will show the first steps, which will be easily extended through the whole.

Exp. 33.

Exp. 33. Upon a *zinc* disk, of the same size as the others, I fixed a piece of *Dutch-gilt paper*, the *copper* side next to the *zinc*, and the *paper* outside, held by a little paste all around. This association is to represent *one group* of the *pile*, considered for the present only on the *copper* side, with the *paper*, which, in the pile, separates it from the next *group* on this side, and more directly from the *zinc* plate of that *group*. Holding on my hand the *zinc* side of the former *group*, I repeated on its *paper*, with the insulated *zinc* disk, the alternate contacts with the condenser; and by 20 repetitions a sensible *negative* divergence was produced in the electroscope of the latter. The same effect takes place in the *pile* at the *copper* side of every *group*; it takes, through the *paper*, some *electric fluid* from the *zinc* of the *next group*; but as *zinc* must always possess more of this *fluid* than the *copper* with which it is connected, the *zinc* of that *next group* takes more from its own associated *copper*, which then, through the *paper*, takes more from the *zinc* of the following *group*; which effect goes on increasing, up to the end of the *pile* on this side. I shall only add to this experiment, that, as the property of a group composed of *zinc*, *copper*, and *paper* on the latter, is to have this side *negative*, whatever be the *metal* with which the above alternate contacts are performed, they produce the same *negative* effect on the *condenser*.

Condenser does not directly show, that the zinc yields electric fluid to the next group:

In order to complete directly the first step here in view, by taking one *group* as an example, it would be desirable, that by placing a piece of paper on the *zinc* side of this *group*, in order to represent the separations of the *groups* in the *pile* in that direction, it should manifest directly by the *condenser*, that *zinc* yields some *electric fluid* to the next *group*; but I have not been able to obtain such a direct proof, on account of a circumstance which I shall now indicate, as peculiar to that quantity of *electric fluid* set in *motion* by the property of the *pile*. This *motion* is very *slow*, compared with the *motion* of an external quantity of *electric fluid*. The smallest degree of electrification of an insulated body, *positive* or *negative*, such as can only affect the gold leaf electroscope, communicated to one side of a *pile*, is instantly manifested at the other extremity; but it is by no means the same with respect to the *electric fluid* set in motion by the property of the *pile*: when the electroscopes diverge equally at its extremities, having touched one of them, which makes the gold leaves fall here, and rise more on the opposite side, it requires a time, often very long, before the same divergence is restored. This is the reason why I was obliged to fix 20 seconds for the duration of the contacts of my piles of 20 groups with the *condenser*, in order to be certain, that the *maximum* of effect was produced; it requires more or less *time*, according either to their nature, or to the extremity applied to the condenser; but I found, that the slowest was produced in 20 seconds, which made me fix that time.

because the motion of the fluid produced by the pile is very slow.

This will explain the case above mentioned, that there is no sensible effect on the *condenser* by any number of alternate contacts of an insulated body with the paper laid on the *zinc* side of one group; and even, when the *groups* are multiplied to increase the effect, it requires, to make it sensible, a prolongation of the contact on the little pile, in order to give *time* for the effect to be propagated; as will be seen by the following experiment.

Exp. 34. Made with a portion of my pile of 10 groups of 1·6 inch diameter, successively tried by each of their extremities, the opposite one being placed on the movable pillar of my condenser, and the alternate contacts being made with a small insulated disk of the same diameter.

Exp. 34.

1. No number of alternate contacts, between either of the extremities of the little pile and the *condenser*, produced any sensible effect on the latter when rapidly made, or in the manner they are executed between the two naked disks, *zinc* and *copper*.

2. On repeating the experiment, with the *zinc* side on the *pillar*, and lengthening the *time* of the contact on the *copper* side up to 4 *seconds*, I found, that by 20 such contacts the

electroscope was affected *negatively* to a certain measurable quantity.

3. Inverting the little pile, I had no sensible effect by these contacts of 4 *seconds* on the *zinc* side, and it was necessary to lengthen the time to 8 *seconds*; but in order to produce on the electroscope a *positive* effect equal to the *negative* of the preceding trial, I was obliged to make 40 contacts, on account of the dissipation of the effects on the condenser in each interval of time. This experiment shows however sufficiently, that the *zinc* side of each *group* yields, through the *paper*, to the next *group*, some of the *electric fluid* that it takes from the *copper* with which it is associated.

Elementary principles of the motion of the fluid in the pile.

We have from these experiments all the elementary principles necessary for the *motion* of the *electric fluid* in the pile, and they are the following.—1. In each *binary group*, the *zinc* plate takes some electric fluid from its associate the *copper*; the latter, in my new *pile*, being the *coppered side* of the *Dutch-gilt paper*. 2. In each group also, *zinc* communicates, through the *paper*, some of its excess of *fluid* to the *copper* of the next *group* on its side. 3. In each *group* again the *copper* takes, through the *paper*, from the *zinc* of the next *group* on its side, some of the *fluid* that it has lost to its associated *zinc*. The same effects taking place in every *group*, with the next on both *sides*, along the whole *pile*, these effects are successively added to those that the respective *next groups* have already undergone according to their *place*: and thus the *negative* state goes on increasing from one end to the other of the *pile*, toward what is called the *copper* extremity; and the *positive* state is increasing toward the *zinc* extremity.

These effects may be represented by *numbers*; though, from the great variations in the quantity at different times, and the imperfection of the electroscopes, these *numbers* remain undetermined: I shall express them in a *pile* of 11 *groups*, indicating by A the *zinc* side, and by B the *copper* side. The two following series represent the progress of *negative* and *positive* effects above mentioned, which, combining in each successive *group* from A to B, constitute, according to circumstances, the three different states of the *pile*, afterward figured.

A

A	A
0	+ 10
— 1	+ 9
— 2	+ 8
— 3	+ 7
— 4	+ 6
— 5	+ 5
— 6	+ 4
— 7	+ 3
— 8	+ 2
— 9	+ 1
— 10	0
B	B

Different states of the pile

In the *insulated* pile, when the *divergence* of the electroscopes is equal on both sides, *positive* at A, and *negative* at B, the state of each successive group is the *sum* of the correspondent *numbers* of the above series, as expressed in the 1st of the following Tables.—When B is placed in communication with the *ground*, the losses of all the *copper* plates being repaired by the latter, all the *acquisitions* of the *zinc* plates subsist without *diminution*, which requires the quantity 10, to be *added* to each number of Table I, as expressed in Table II.—When A communicates with the *ground*, all the *acquisitions* of the *zinc* plates being carried into the latter, the *losses* of the *copper* plates remain uncompensated, and the same quantity, 10, is to be *substracted* from all the numbers of Table I, as expressed in Table III.

Table I.	Table II.	Table III.
Insulated pile.	B *in com. with the gr.*	A *in com. with the ground.*
A	A	A
+ 10	+ 20	0
+ 8	+ 18	— 2
+ 6	+ 16	— 4
+ 4	+ 14	— 6
+ 2	+ 12	— 8
0	+ 10	— 10
— 2	+ 8	— 12
— 4	+ 6	— 14
— 6	+ 4	— 16
— 8	+ 2	— 18
— 10	0	— 20
B	B	B

Now this synthesis of the above fundamental experiments is the real fact; as will be seen from direct experiments in the following paper, of which I shall give here only the general results.

proved by experiment.

For these experiments I use a horizontal *pile*, which I have called *column*, with a gold leaf electroscope at each extremity; and I have also a detached electroscope, which may be applied and observed at every point of the *column*. The following are the observed phenomena.—1. When the state of the ambient air is such, that in the *insulated column*, the divergence is equal at both extremities, the *middle* point in its length is *zero*, as represented in Table I, in which, as well as in the two others, the *terms* are to be considered only as equidistant *points*, whatever be the number of the *groups*.—2. When B communicates with the *ground*, the first plate only at this extremity is *zero*, and the *positive* state is gradually increasing towards A: the *middle point* is *plus*, of the same quantity (sensibly) as it was at A in Table I, and the divergence *plus* is doubled at A; a state represented in Table II.—3. When A communicates with the *ground*, all the effects are reversed: the first plate only at A is *zero*, and the *negative* state is gradually increasing towards B: the *middle point* is now *minus*, of the same quantity (sensibly) as it was at B in the case of Table I, and the quantity *minus* is doubled at B; a state represented by Table III. I do not know any theory on *invisible causes*, which more exactly follows the *visible effects*.

Physical cause of these phenomena.

What remains to be considered is the *physical cause* of these phenomena, all originating in this circumstance, that when *zinc* and *copper* are in mutual contact, *zinc* possesses more *electric fluid*, and *copper* less, than in another situation. In my first paper delivered to the Royal Society, I explained this effect by analogy with the phenomenon of the different *capacities* of bodies for the *fluid* cause of *heat*; but having here entered into an explanation of the nature of the *electric fluid*, I shall derive analogies from the subject itself.

Most electric phenomena depend on the density of the fluid.

Most of the electric phenomena manifested in our experiments depend on the distinction, which I have established, between the *density* of the *electric fluid*, consisting in the proportional quantity of *electric matter* and its *expansive power*,

power, depending on the quantity of *vector.* This distinction is particulary manifested by the *changes*, that happen along an insulated *conductor* of some length, when an electrified body is placed at some distance from one of its extremities. It is known in general, that the extremity of such a *conductor* next to the *electrified body* acquires an *electric state* contrary to that of the *body*, while its opposite extremity has the same electric state as that body: for instance, suppose the electrified body to be *positive*; it is commonly said, that the extremity of the *conductor* next to this *body* becomes *negative*, and the opposite extremity *positive*; but these are vague expressions, and as they give no real idea of the effects, they have occasioned the variety of systems, all unsatisfactory and therefore changing, hitherto made on these phenomena.

Distinction between its expansive power and density necessary.

The cause of obscurity on this object is the want of that distinction above mentioned, between the *expansive power*, and the *density* of the *electric fluids*, belonging to its nature, as *expansible.* Every fluid of this class, when confined within a certain space, has necessarily the same degree of *expansive power* in every part of this space, since this is attached to the very idea of *expansibility*; but it is not the same with respect to *density.* For instance; a mass of *air*, confined in a certain space, has certainly, in all its parts, the same *degree* of *expansive power*, whatever change may happen in its partial *density.* If then a *hot body* be placed near one side of this mass of *air*, its *density* will *diminish* in this part, and *increase* in the more remote: or if a piece of *ice* be brought on one side of this mass of *air*, its *density* will *increase* near the *ice*, and *diminish* in the remote parts; but the *degree* of *expansive power* of this confined *air* will always be equal in all its parts at the same time, *increasing* or *diminishing* in the whole.

Illustrated by the action of heat.

Instanced in electricity.

The case is exactly the same with respect to the *electric fluid* on an insulated *conductor:* an insulated *positive* body being placed near one part of the latter, this part receives some of the *vector* which forms around the *positive body* a kind of atmosphere, as the *hot body* has around it an atmosphere of *igneous fluid*; and the former produces an increase of the *expansive power* of the *electric fluid* on this part of the *conductor*, as the *hot body* produces it in the *air* next to it. A *negative body* produces also an effect analogous to a piece of *ice:* for as this absorbs a part of the free *fire* in the parts of the mass of *air* next to it; so the *negative body* absorbs a part of the *vector* from the *electric fluid* on the nearest part of the *conductor*. Now, all these changes in the *degree* of *expansive power* in both *fluids* are attended with inverse changes in their *density*.

This proved by experiments.

Nobody will doubt of the above statement, who has repeated the experiments described in my works; and I may say, that every one of the great number of persons, in whose presence I have made these experiments, with the set of small electric instruments described in my *Traité élémentaire sur le Fluide Electro-galvanique*, has been convinced of this important distinction between the *density* and *expansive power* of the *electric fluid*. It is not therefore for want of progress in the science itself, that such variety of systems subsists concerning the *electric phenomena;* it is for want of attention in most experimental philosophers, by whom, though writing on *electricity*, my published experiments are never mentioned, not so much as to criticise them, or their *conclusions*. Among the variety of these experiments, I have here chosen those concerning the modifications which take place on a long insulated *conductor*; and I come now to the particulars, which will prove all that I have stated above.

Three *electroscopes* are used in that series of experiments, one of which, a *movable* one, consists of silver *laminæ*, suspended on small axes, in order to prevent their motion out of the line of their divergence. Of the two other *electroscopes*, consisting of small pith balls suspended by silver wires, one is permanently connected with the extremity of the *conductor* farthest from the *electrified body*; the other, held up at the top of a high insulating pillar, is kept, by means of a thin wire, in constant communication with the *movable electroscope:* the latter, though principally destined to move along the *conductor*, may be removed, from every point, to a distance, in order to try the *nature* of the *divergence* that it had at this point, which it does not lose on removal. Lastly an insulated brass *disk* is the *electrified body*. The following are the phenomena observed, in which

Points to be proved.

must be recollected what I undertake to prove, namely, that the *divergences* of the *electroscopes* are produced by *plus* or *minus* of *electric matter* only, comparatively with the *standard*, which is the *actual* proportional quantity of *electric matter* possessed by the *ambient air*; an object connected with the proof of the fundamental proposition, that there is an absolute distinction between the *density*, and the *expansive power* of the *electric fluid*.

Phenomena.

1. At the beginning of the experiment, the *movable electroscope* is placed near that extremity of the *conductor*, to which the disk, after having received a spark from a Leyden vial, is to be approximated. At the approach of this *positive body* within a small distance, the silver *laminæ* of this *electroscope* diverge as *negative*; some of their *electric matter* having receded to the remote parts of the *system* (by this word I express the *conductor* and its associated *electroscopes*); and thus, though the *expansive power* of the *electric fluid* has equally increased upon its whole extent, the two remote *electroscopes*, and in particular that which is connected with the *silver laminæ*, diverge *positively* by a certain quantity.

2. When the *silver laminæ* are made to *recede* from the *positive* body along the *conductor*, their *negative* divergence gradually diminishes; it ceases at a certain distance, and farther than this begins a *positive* divergence, which increases to a certain *maximum*; nevertheless the remote electroscopes, and in particular that which is in immediate connection with the *laminæ*, remain *positive* to the same degree as at first. The effect, therefore, of withdrawing the *laminæ* has been to remove them out of the atmosphere of *vector* of the *positive body*, which, by increasing the *expansive power* of their *electric fluid*, had made a part of their *electric matter* to abandon them: and it is the *electric matter* retired from the anterior part of the *system*, which occasions the *positive* divergence in the remóte *electroscopes*.

3. While the *movable electroscope* is thus removed from the atmosphere of the *positive body*, if any part of the *system* be touched with a small wire held in the hand, the *expansive power* of its *electric fluid* is thus placed in *equilibrium* with that of the *ground*, and the *divergence* ceases in all the *electroscopes*. Which shows, that, in the parts of the *system* over which the atmosphere of the *positive body*

does not extend, the *equilibrium* of proportional quantity of *electric matter* has been also produced. If then the *silver laminæ* be moved toward the *positive body*, when they arrive within that atmosphere they begin to diverge as *negative*; and this divergence continuing to increase, it is greater when this electroscope arrives at the end of the *conductor*, than it had been in the same place at the beginning of the experiment. However, the remote *electroscope* in communication with this remains without divergence; because the small quantity of *electric matter* newly withdrawn from the *silver laminæ* is insensible upon the whole *system*.

4. Now will come a proof (among many others which may be found in my works), that the *divergence* in the electroscopes depends *only* on the proportional quantity of *electric matter*, or *density* of the *fluid*. If, in this state of the *system*, the *silver laminæ* be touched with a wire held in the hand; though this contact places them in communication with the *ground*, their *divergence* continues the same: because their *electric fluid*, by the increase of *vector* proceeding from the *positive body*, being *in equilibrium* of *expansive power* with that of the *ground*, no *electric matter* can ascend to them from the latter, and they remain *deprived* of it to the same degree.

5. Lastly. If the *positive body* be removed or discharged, the three electroscopes diverge as *negative*. By the contact of the *system*, during the influence of the *positive body*, which had increased the *expansive power* of the *electric fluid* over the whole, the quantity of *electric matter*, which had retired from its anterior parts, had passed into the *ground*; and now, when the influence of the *positive body* has ceased, this *deficiency* of *electric matter* becomes common to the whole system.

Fundamental proposition.

Having now, by this series of experiments, demonstrated the fundamental proposition, that an *equilibrium* of *expansive power* of the *electric fluid* may subsist, between two insulated bodies in mutual contact, with a difference in its *density*, or proportional *quantity* of *electric matter*, I come to my system of the *electric states* of *zinc* and *copper*, when, being insulated, they are in mutual contact, which system is founded on that proposition.

System of the

It is evident, that the *electric fluid* must be in *equilibrium*

brium of *expansive power* on this group composed of *zinc* and *copper*; therefore, the *difference* observed in their *electric states* must proceed from a difference in the *density* of the *fluid*. Now, the only hypothesis added in my system to this immediate conclusion from fact is this: that, during their connexion, *copper* has the property of acquiring more *vector* than *zinc*, from that diffused in the *ambient air;* by which proportional increase of *expansive power*, the *electric fluid* on *copper* is in equilibrium with that on *zinc*, though with less *density*, or proportional *quantity* of *electric matter*. I have shown also in the above mentioned works, by deduction from experiments, that, when this *influence* of bodies on each other, by *plus* or *minus* of *vector*, has ceased by sufficient *distance*, each of them possesses instantly, by the effect of the *ambient medium*, a quantity of *vector*, proportional to its quantity of *electric matter*; and thus it is, that the modifications produced by *zinc* and *copper* on each other, while associated, and their effects on bodies brought into contact with them on the outside of their *groups* during their association, are converted into modifications of the *quantity* of the *electric fluid* itself.

electric states of insulated zinc and copper in contact.

After having treated here the theoretical part of the subject more fully than I had done (for brevity's sake) in my first paper given to the Royal Society; in order to be better understood on this subject, very important in natural philosophy; I return to the experiments concerning the *analysis* of the *galvanic pile*, to bring them here to the same point as they were at in that paper.

Contrivance for employing a great number of small plates.

Having found, by the experiment related at the end of the first *part* of this analysis, that, by increasing the *size* of the *plates*, the *divergence* did not increase in the electroscopes, I considered the manner in which a great number of small plates might be used. I thought then of having a hole in the centre of small plates, in order to thread them with silk in form of *chaplets*, alternating the plates with equal pieces of *Dutch-gilt-paper*. Not having yet any but *tinned iron plates* for these trials, I formed two such *chaplets*, each composed of 140 *groups*, of 0·5 inch diameter, and in order to guard them against dust, I enclosed them in glass tubes: but I found, that, when the *chaplets* lay along the glass, the effect was diminished, and in order to prevent this defect, I took larger tubes, with metallic caps, through which, by screws, I kept the *chaplet* fixed in the axis of the tube; these screws, being on the outside in the form of hooks, served to link the *chaplets* together. Each of these small instruments acted immediately on the gold leaf electroscope, and, by hooking them together, the effect was doubled.

I founded at that time, on this experiment, the plan of increasing the power of the new instrument so as to produce the divergence of small metallic balls, especially by using *zinc* plates, which I knew then could be procured. For this purpose, I thought of lessening the expense occasioned by the metallic caps for the glass tubes, by making much longer *chaplets*, and of suspending these in the form of garlands, to the ceiling of the room, by silk strings, bringing only wires from their opposite extremities to a proper place, where the apparatus of the small metallic balls should stand.

Electric column and aerial electroscope.

Such is the point at which I had arrived the 30th of May, 1808; and this object was much forwarded in my paper under the title of *The Electric Column and Aerial Electroscope*, delivered to the Royal Society the 7th of March, 1809; but the Committee of Papers not having ordered it to be published in the Phil. Trans., it will now appear in a more advanced state. The consequence of its not appearing at the time it should have done is, that my *electric column* has lost the merit of novelty; for, by the communications of the minutes at the meetings of the Society and the Committee, it had attracted attention; and the sight of it, which I did not refuse, has made it sufficiently known to be already imitated. However, while its connexion with the analysis of the Galvanic pile is not considered, its principal object is lost: and moreover, till the *sticking* of the small *metallic pendula*, when *striking*, was prevented, which I have obtained but lately, it could not be ranked among *meteorological* instruments. These particulars will be seen in my following paper.

Windsor, 22d of June, 1810.

II.

On the Botryolite, or Grapestone: by Count DUNIN BORKOWSKI*.

Botryolite.

THIS stone is only found in mass, and its external figure is uniform. From this figure its name is derived.

Its characters.

Its colours are pale rose, pearl gray, yellowish white, ashen gray, and Isabella yellow. All these colours alternate in very thin laminæ; the deepest colour in general forming the outermost coat.

Both externally and internally it is without lustre.

Its fracture is with slender, divergent fibres. It becomes scaly.

Sometimes it is translucid throughout, sometimes only at the edges.

It is semihard, scratching glass only in a slight degree; and brittle.

Its specific gravity is 3·000.

Before the blowpipe it is fusible with ebullition.

Contains boracic acid.

It contains boracic acid.

Where found.

It is found accompanied with quartz, black schorl, carbonate of lime, martial pyrites, and magnetic iron, in the mine of Kienlie, near Arendahl, in Norway.

III.

An Analysis of several varieties of British and Foreign Salt (Muriate of Soda), with a view to explain their Fitness for different economical purposes. By WILLIAM HENRY, *M. D. F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester.*

(Concluded from p. 206.)

SECT. III. *Account of the Methods of Analysing the several Varieties of Muriate of Soda.*

THE method of analysis, which I adopted, in examining the several varieties of muriate of soda, was as follows.

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* Journal de Physique, vol. LXIX, p. 159.

Method of analysing the muriate of soda.

When the salt was in a state of solution, a measured quantity was evaporated to dryness in a sand heat, which was carefully regulated, to avoid the decomposition of the muriate of magnesia, if any of that salt were present in the solution*.

Each specimen of salt was reduced to a fine powder, and was dried, in the temperature of 180° of Fahrenheit, during the space of two hours. This was done in order that the different experiments might be made on precisely equal quantities of salt.

I. *To separate the earthy Muriates.*

Separation & calculation of the earthy muriates.

(A) On 1000 grains of the dried and pulverized salt, (except in the case of the foreign salts, when only 500 grains were used,) four ounce measures of alcohol were poured, of a specific gravity varying from 815 to 820, and at nearly a boiling temperature. To insure the access of the fluid to every part of the salt, they were ground together for some time in a mortar, and then transferred into a glass matrass, where they were digested for some hours, and frequently agitated. The alcohol was next separated by filtration, and the undissolved part was washed, as it lay on the filtre, with 4 ounce measures of fresh alcohol.

(B.) The united washings were evaporated to dryness†, and to the dry mass a small portion of fresh alcohol was added, to separate the earthy muriates from a little common salt, which had been dissolved along with them. This second solution might, however, still contain a minute portion of muriate of soda. It was therefore again evaporated, redissolved in hot water, and mixed with a solution of carbonate of soda. By boiling for some minutes, the whole of the earths were precipitated, and after being well washed, were redissolved in muriatic acid. This solution, being evaporated to dryness, gave the weight of the earthy muriates, which had been extracted by alcohol*.

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* Muriate of magnesia, according to Dr. Marcet, begins to part with its acid at a temperature a few degrees above that of boiling water. This fact explains the observation of Mr. Kirwan, that too great a heat, employed in the desiccation of muriate of magnesia, decreases considerably its solubility in alcohol. (Kirwan on Mineral Waters, p. 275.)

† In this and all similar cases, the heat was very cautiously regulated towards the close of the process.

(B. a.) The dry mass thus obtained might consist either of muriate of magnesia, or muriate of lime, or of both. An aliquot part, therefore, was dissolved, separately, for the purpose of assaying it by the usual tests. Sometimes, as in the case of the earthy muriates procured from sea salt, muriate of magnesia alone was indicated, and any farther process was rendered unnecessary. Muriate of lime was in no instance found uncombined; but in the majority of cases (as in the earthy muriates obtained from Cheshire salt) was mixed with muriate of magnesia.

(B. b.) To the solution of the two earthy muriates was added fully saturated carbonate of ammonia, which has the property of throwing down lime in combination with carbonic acid, but has no effect on the muriate of magnesia at ordinary temperatures. The solution of the latter salt, along with that of the excess of carbonate of ammonia, was therefore separated by filtration; and to the filtered liquor a solution of phosphate of soda was added, according to the formula of Dr. Wollaston†.

(B. c.) By direct experiments I had learned, that 100 grains of muriate of magnesia, when thus decomposed by carbonate of ammonia, conjoined with phosphate of soda, give 151 grains of insoluble ammoniaco-magnesian phosphate dried at about 90° of Fahrenheit. Hence it was easy, from the weight of the precipitate, to calculate how much of the former salt was contained in the mixture of muriate of lime and muriate of magnesia. Thus, if 20 grains of a mixture of the two muriates yielded 15·1 of ammoniaco-magnesian phosphate, it is obvious, that the mixture must have consisted of equal weights of muriate of lime and muriate of magnesia.

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* By the analysis of artificial mixtures of pure muriate of soda with the earthy muriates in known quantities, I afterward found, that the full amount of the earthy muriates was not ascertained in this way of proceeding. The deficiency of the latter salts was about one sixth; but as the errour must necessarily have been the same in all, it does not affect the comparison of different varieties of salt, as to their proportion of this ingredient. If the numbers in the 5th column of the table (indicating the total earthy muriates) be increased in the proportion of six to five, we shall then obtain the true quantities in each variety of salt.

Full amount of the earthy muriates given too small.

† See Dr. Marcet's analysis of the Brighton Chalybeate, published in the last edition of Saunders on Mineral Waters.

(B. d.) The estimation of the proportion of muriate of lime, in a mixture of this salt with muriate of magnesia, was sometimes performed in a different way. To a cold solution of a known weight of the two salts, superoxalate of potash was added; and the precipitate was collected, washed, and dried at about 160° Fahrenheit. Of this precipitate I had previously found, that 116 grains are formed by the decomposition of 100 grains of dry muriate of lime. From the quantity of oxalate of lime it was easy, therefore, to infer that of the muriate, from the decomposition of which it resulted; and this, subtracted from the weight of the two salts, gave the weight of the muriate of magnesia.

II. *To separate and estimate the earthy Sulphates.*

Separation and calculation of the earthy sulphates.

(C.) The portion of salt, which had resisted the action of alcohol, was dissolved by long boiling in sixteen ounce measures of distilled water, and the solution was filtered. On the filtre a small quantity of undissolved matter generally remained, which was washed with hot water, till it ceased to have any action. The weight of the insoluble portion was then ascertained.

(C. a.) By this operation were dissolved, not only the muriate of soda, but all the other salts, insoluble in alcohol, which might be mingled with it. To the solution carbonate of soda was added; and the liquid, which in most cases gave, on this addition, an abundant precipitate, was boiled briskly for several minutes, in order that none of the earthy carbonates, which were separated, might remain dissolved by an excess of carbonic acid.

(C. b.) The precipitated earths were allowed to subside, and were well edulcorated with boiling water, the washings being added to the liquor first decanted from the precipitate. To these united liquids (after the addition of more muriatic acid than was required for saturation) muriate of barytes was added, till it ceased to occasion any further precipitate.

The

Separation and calculation of the earthy sulphates.

The sulphate of barytes was then washed sufficiently; dried, ignited, and its amount ascertained.

To the earthy carbonates an excess of sulphuric acid was added in a platina dish, and the mixture was triturated, till all effervescence ceased. It was then evaporated to dryness, calcined in a low red heat, and the weight of the earthy sulphates was ascertained.

(D. a.) The dry sulphates were washed with a small quantity of lukewarm water. In several instances, the loss of weight, thus sustained, was extremely trifling, nothing being dissolved but a very minute portion of sulphate of lime, of which earthy salt, solely, the residue was presumed to be composed.

(D. b.) But in other cases, a considerable loss of weight ensued; and in these, to the watery solution was added a mixture of equal parts of saturated solutions of carbonate of ammonia, and phosphate of soda. A precipitate more or less copious was produced, which was collected, dried at 90° Fahrenheit, and weighed,

(D. c.) By direct experiments I had determined, that 90 grains of this precipitate result from the decomposition of 100 grains of sulphate of magnesia, of such a degree of dryness, as to lose 44 grains out of 100, by exposure to a low red heat. Hence 100 grains of ammoniaco-magnesian phosphate indicate 111 grains of crystallized, or 62·2 of desiccated, sulphate of magnesia*. From the weight of the ammoniaco-magnesian phosphate, it was easy, therefore, to infer the proportion of sulphate of magnesia in any mixture of the two earthy sulphates.

(D. d.) It was possible, however, that, in addition to the sulphates of lime and of magnesia, the quantity of which had been determined by the foregoing process, the specimen of salt under examination might contain also an alkaline sulphate. To decide this point, it was necessary to compare

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Water of crystallization in sulphate of magnesia.

* The assumption, that crystallized sulphate of magnesia contains only 44 per cent of water, though it was correctly true with the specimen on which I operated, is below the average; which, I find from several experiments, is about one half the weight of the salt. Mr. Kirwan states the water of crystallization to be 53·6 in 100 grains; but this, I believe, a little exceeds the truth.

Separation and calculation of the earthy sulphates.

compare the amount of the acid, deducible from the weight of the sulphate of barytes (C. b.), with that which ought to exist in the sulphate of lime and sulphate of magnesia, actually found by the experiment. But, to make this comparison, some collateral experiments were previously necessary.

(D. e.) By these experiments I found, that sulphate of lime prepared by double decomposition, then calcined in a low red heat, and afterward dissolved in a large quantity of boiling distilled water, yields, when precipitated by a barytic salt, in the proportion of 175·9 grains of sulphate of barytes from 100 of the calcareous sulphate*. The same quantity of ignited sulphate of lime (= 128 grains dried at 160° Fahrenheit,) precipitated by superoxalate of potash, gives 102·5 of oxalate of lime; or, precipitated by subcarbonate of potash at a boiling heat, 74·3 grains of carbonate of lime†. One hundred grains of crystallized sulphate of magnesia (= 56 desiccated) afford, when precipitated by muriate of barytes, 111 or 112 of the barytic sulphate.

(E.) By a comparison of the above proportions with those obtained in the analyses of any specimen of common salt, we may learn, whether it contain other sulphates beside those with earthy bases. For example, if the precipitate (D.) consist of carbonate of lime only, and bear to the sulphate of barytes (C. b.) the proportion of 74 to 175, or very nearly so, we may infer, that no other sulphate is present, but that of lime. The same conclusion will follow, if, after having decomposed one half of the watery solution (C.) by muriate of barytes, and another half by potash, we find

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* This result corresponds, within a fraction of a grain, with one obtained in a somewhat different way by Dr. Marcet; and very nearly with an experiment of my friend Mr. James Thomson, who found the barytic sulphate, precipitated from 100 grains of sulphate of lime by nitrate of barytes, to weigh 173 grains.

† On reversing this experiment, I found that 100 grains of carbonate of lime, saturated with sulphuric acid, and calcined in a low red heat, afford 135 of sulphate of lime. A similar experiment of Mr. Thomson gave 134·6 grains. Dr. Marcet, also, informs me, that from 93·55 grains of pure marble he obtained 125·95 grains of sulphate of lime, proportions which exactly coincide with those of Mr. Thomson.

Separation and calculation of the earthy sulphates.

that the sulphate of barytes bears to the oxalate of lime the proportion of 175·9 to 102·5. Now these proportions were, as nearly as could be expected, obtained in the analysis of Northwich salt; whence we may conclude, that the only sulphate, which it contains, is gypsum, or the sulphate of lime.

It must be remembered, however, that the calcareous sulphate, contained in any variety of common salt, cannot be in a state of complete desiccation, but would lose 22 parts out of 100, by exposure to a red heat*. It becomes necessary, therefore, either to increase, in the proportion of 5 to 4, our estimate of the sulphate of lime obtained by the foregoing rule, or, more simply, to assume, that 100 grains of sulphate of barytes indicate 73 grains of sulphate of lime, dried at 160° Fahrenheit, = 57 ignited.

(F.) When sulphate of lime and sulphate of magnesia were both ascertained, and other sulphates also might possibly be present, as in the varieties of salt from sea water, the calculation became a little more complicated. In this case, after determining the quantity of both sulphates, (by the processes D. &c.) I estimated how much sulphate of barytes they ought respectively to afford; and then compared the estimated quantity with that which was actually obtained. The earthy carbonates, for example, precipitated from 1000 grains of Lymington salt, which had previously been digested with alcohol, were converted into 31 grains of calcined sulphates, consisting of 19 grains of dry sulphate of magnesia, and 12 grains of dry sulphate of lime. Now from the magnesian sulphate 38 grains of sulphate of barytes should result; and from the sulphate of lime 21 grains; the sum of which is 59. But the quantity actually obtained was 59·8. There is only, therefore, an excess of 0·8 grain of the actual above the estimated quantity, a difference much too trivial to be admitted an indication of any sulphate with an alkaline base; and arising, probably, from unavoidable errours in the experiment.

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* This I find to be the loss sustained by 100 grains of artificial selenite, dried at 160°, and then ignited. The same quantity of crystallized native selenite, I learn from Dr. Marcet, loses 20·7 grains, by being calcined in a strong red heat.

(F. a.) If in any mixture of salts, free from the earthy muriates, we are certain that no other sulphates exist beside those of lime and magnesia, their estimation becomes extremely simple. Decompose two equal quantities of the salt in question, the one by muriate of barytes, the other by oxalate of potash. From the weight of the latter precipitate we may calculate the quantity of sulphate of lime. Suppose, for example, the oxalate of lime (as was actually the case with the precipitate from 1000 grains of Lymington salt,) to weigh 12 grains: these denote 15 of sulphate of lime, dried at 160° Fahrenheit; which quantity, if decomposed, would give 20½ of sulphate of barytes. The latter number (20½), subtracted from the weight of sulphate of barytes actually obtained (say 60), gives 39½ grains for the sulphate of barytes resulting from the decomposition of the sulphate of magnesia. The quantity of the latter salt, it will be found, therefore, by applying the rule already given (D. e.), must be 35 grains.

(F. b.) The same object may be accomplished by decomposing two equal quantities, the one by oxalate of potash, the other by the compound solution (D. c.) From the weights of the precipitates it is easy to calculate, from how much of the calcareous and magnesian sulphates they have resulted.

Separation of alkaline sulphates.

(G.) When the salt left by alcohol was known to contain muriate of soda, and sulphate of magnesia, but no sulphate of lime, the presence of alkaline sulphates was investigated in the following manner. The salt was dissolved in water, and the solution was divided into two equal portions. To the one muriate of barytes was added, and to the other, the compound precipitant of carbonate of ammonia, and phosphate of soda. If the sulphate of barytes, thus produced, bore to the ammoniaco-magnesian phosphate the proportion of 112 to 90, it was concluded, that no other sulphate had been decomposed, but that with base of magnesia.

(H.) At one time, I expected to have ascertained the quantity of sulphate of soda, in an artificial mixture of that salt with sulphate of magnesia and muriate of soda, by the following formula. To a solution of the three salts, heated to a boiling temperature, I added subcarbonate of ammonia, which decomposes the sulphate of magnesia only. I had then a solution containing muriate and sulphate of soda, with sulphate of ammonia, and some carbonate of ammonia. This solution was evaporated to dryness, and the mass was sufficiently heated to expel the ammoniacal salts. I found, however, that at this temperature the sulphate of ammonia acted upon the muriate of soda, and produced an additional, and not inconsiderable quantity of sulphate of soda.

Impurities deducted gave the weight of muriate of soda.

Having determined, by the foregoing processes, the quantity and kind of the earthy muriates, the amount of the insoluble matter, and the proportion of sulphates, the weights of all these different impurities were added together; and, the sum being deducted from the weight of the salt submitted to experiment, the remainder was assumed as the amount of the pure muriate of soda in the specimen under examination*.

Additional circumstances.

Though I purposely refrain from giving the details of the several analyses, which were made according to the foregoing plan, from the conviction that they would be both tedious and unnecessary, yet there are a few circumstances, which it may be proper to mention more fully than can be done in the form of a table.

Brine from Northwich.

1. The *brine* which I examined was from Northwich, and was sent me in the state in which it was taken from the spring†. At the temperature of 50° Fahrenheit, it had the specific gravity of 1·205. It was perfectly limpid, but lost a

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* I have deemed it unnecessary to state, in the table, the quantities of the acid and base in several varieties of muriate of soda. They may readily be estimated from the proportion, deduced by Dr. Marcet, of 46 acid, and 54 soda, in 100 of the pure muriate. In this determination he assumes, that 100 parts of luna cornea, after being melted and heated to redness, consist of 19·05 parts of acid to 80·95 oxide of silver. This statement agrees very nearly with the recent one of Gay-Lussac, who makes 100 parts of silver to combine with 7·60 oxigen, and this oxide to neutralize 25·71 parts of real muriatic acid.

† I have lately been informed, that this brine had been pumped out of a rock-salt mine, into which, from the impossibility of obtaining the salt in a solid form, it was allowed to flow. Hence it was fully saturated with muriate of soda.

little of its transparency when raised to a boiling heat, in consequence of the deposition of a very minute quantity of carbonate of lime, and oxide of iron. It was immediately precipitated by muriate of barytes, oxalate of ammonia, and alkaline solutions, both mild and caustic. Eight ounce measures, evaporated to dryness in a sand heat, gave 1230 grains of salt, which, for the sake of distinction, I term *entire* salt. It proved, on analysis, to contain in one thousand parts*,

Composition of its salt.

Carbonate of lime and oxide of iron	2
Muriate of lime, and muriate of magnesia, in nearly equal proportions	5
Sulphate of lime	19
Muriate of soda	974
	1000

Mother liquor.

2. The *mother liquor*, or brine that remains after separating all the common salt, which it is thought worth while to extract, had the specific gravity of 1208. The dry salt contained

Muriate of magnesia	35
lime	32
Sulphate of lime	6
Muriate of soda	927
	1000

Clearings of the brine.

3. The *clearings* of the brine, which are raked out of the pan when the salt first begins to granulate, contained in 1000 parts,

Muriate of soda	800
Carbonate of lime	41
Sulphate of lime	159
	1000

Pan scale.

4. Of the substance called by the workmen *pan-scale* two specimens were analysed, the one containing a large propor-

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* The specific gravity and proportion of earthy sulphates in Cheshire brine appears to differ considerably in the brine of different springs. See Holland's Cheshire Report, p. 45, &c.

tion of muriate of soda, the other very little. The first variety consisted of

Muriate of soda ········	950
Carbonate of lime ······	10
Sulphate of lime ········	40
	1000

The second variety was composed of

Muriate of soda ········	100
Carbonate of lime ······	110
Sulphate of lime ········	790
	1000

Circumstances, however, are constantly occurring, to vary the proportion of ingredients, both in the clearings and in the pan-scale. If, for example, the brine be short of the point of saturation with common salt, it acts, when admitted into the pan, upon the muriate of soda which the pan-scale contains, and we obtain the second variety. But if the brine be fully charged with salt, it affects no solution of the muriate of soda carried down along with the gypsum; and then the first species of pan-scale results.

Salt oil, or mother liquor of seawater.

5. The *salt oil*, or mother liquor from seawater, a specimen of which I received from Dr. Thomson, had the specific gravity of 1277. It was abundantly precipitated by muriate of barytes: by pure ammonia, but not by the carbonate; and was not changed by oxalate of potash, either immediately or after an interval of some hours. One thousand parts of the dry salt consisted of

Muriate of magnesia ····	874
Sulphate of magnesia····	70
Muriate of soda ········	56
	1000

Salt brine, or liquor drained from Scotch salt.

6. The *salt brine*, or liquor which drains from the Scotch salt, had the specific gravity of only 1188. It was affected by the same tests as the salt oil, but less remarkably. The dry residue contained

Muriate

Muriate of magnesia	205
Sulphate of magnesia....	135
Muriate of soda	660
	1000

Mother liquor, or bittern, from Lymington.

7. The *mother liquor*, or *bittern*, *from Lymington*, presented, on analysis, an unaccountable variation from the similar fluid sent from Scotland, and gave a much larger proportion of sulphate of magnesia. A considerable quantity of this salt had, moreover, crystallized in the bottle which contained the liquid. Its specific gravity was 1280. One thousand parts of the dry salt contained of

Muriate of magnesia	640
Sulphate of magnesia	260
Muriate of soda	100
	1000

Pan scale from Lymington.

8. The pan-scale from Lymington contained

Muriate of magnesia	29
Dessicated sulphate of magnesia	18
Carbonates of lime and magnesia*............	127
Sulphate of lime........	216
Muriate of soda	610
	1000

No sulphate of soda present.

From the very near approximation of the proportions between the sulphate of barytes and ammoniaco-magnesian phosphate, obtained in the analysis of all these products of seawater, to those which result from the decomposition of two equal quantities of sulphate of magnesia, it may be inferred, that they contain no sulphate of soda[*]. For example, to decide whether the Scotch salt contains an alkaline sulphate, or not, I dissolved 1500 grains in a pint of boiling water, and evaporated till fourteen drachm measures only remained, the common salt being removed as soon as it was formed. The residuary liquid was divided into two equal portions, one of which gave 18½ grains of sulphate of barytes, and the other, 14 grains of ammoniaco-magnesian phosphate. The proportion between these numbers is so nearly that which has been already assigned, (viz. 112 to 90,) that we may safely infer the total absence of sulphate of soda. This salt, indeed, is considered as incompatible with muriate of magnesia; but after digesting, for two or three days, 100 grains of the former, with 20 of the latter, evaporating to dryness, and washing the residuum with repeated effusions of alcohol, I found, that two grains of the muriate of magnesia had escaped decomposition.

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* The proportion of these carbonates I was prevented from determining by an accident.

IV.

*On the Action of the Electric Fluid, by which an Iron Cylinder an Inch and half thick was torn asunder: in a Letter from Mr.*** to* J. C. DELAMETHERIE[†].

SIR,

Metallic cylinders burst by electric shocks.

I Have already communicated to you, in different letters, my experiments on various metallic cylinders, that were torn by electric discharges. I half filled with water the cylinder, Plate VII, fig. 1, which was open only at one end. In this water I immersed a small leaden rod, which I surrounded with wax at the place where it entered the cylinder, in order to insulate it, as at fig. 3. I then formed a communication between one of the surfaces of a powerful elec-

tric

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[*] I employed more attention in investigating the presence of sulphate of soda in the products of seawater; because this salt is stated to be one of its ingredients by the Bishop of Llandaff, (Chemistry vol. ii. p. 62,) and by other chemical writers.

[†] Journ. de Physique, vol. LXVIII, p. 411.

Explosions.

tric battery and the bottom of the cylinder, and between the other and the leaden rod. The explosions were very strong; the water being frequently ejected with violence, and the leaden rod thrown to a distance. After several explosions the cylinder was more or less torn.

Application to natural phenomena.

Reading in your Journal for january, 1809, the reflections, with which you conclude the article of earthquakes, and the influence that water penetrating through the clefts of the mountains must have on them, I considered the electric action of our apparatuses as an imitation of what passes in the mountains where these phenomena take place. The electric fluid acts in its usual manner on the inflammable substances contained in them, and produces on them considerable effects. My preceding experiments had shown, that this fluid could burst metallic cylinders of considerable strength; and I was willing to try, whether it could not produce still greater effects. Accordingly I made a cylinder of iron, fig. 1, twenty-seven lines high*, and eighteen in diameter; with a hole in the centre, *n*, 1¾ line in diameter, and 18 deep. It was subjected to the same experiments as the former cylinders, and was burst by seventy explosions.

Experiment with an iron cylinder, near three quarters of an inch thick.

Battery of 100 feet

The battery I employed had 100 feet of metallic coating. At every discharge the opening was well cleaned with an iron wire and water, let fall into it drop by drop, to remove all the oxide of lead and iron, which was separated at each discharge in half fused and oxided grains. Then, having emptied out the water by shaking the cylinder, and filling it with fresh water, the wires ready prepared being placed in the opening, the little apparatus was placed in a box, and, Henley's electrometer indicating an intensity of 60° or 65°, the discharge was made. Seventy discharges produced the rent *a*, fig. 1. It required about a quarter of an hour to charge the battery; so that two intelligent workmen could obtain about twenty discharges a day, even in my absence.

burst it in 70 discharges.

I beg you to get the cylinder sawn in a direction opposite to the rent *a*, till you come to the hole *n*, in order to observe the effects of this rupture within†.

In

Appearance of

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* French measure. The dimensions are the same in the plate. C.

† I requested Mr. Dumoutiez, who is well experienced in the construction

Strongest effect when the fluid is directed to one point.

In my former experiments I had observed, that, when the electric fluid was constantly directed to one point, the metallic decomposition sooner took place. In consequence I took care to make the cylinder rest on its centre, raising the metallic plate a little at a single point, and surrounding this point with wax. The effect was so striking, that at the 70th discharge the opening was in the state represented at fig. 1 and 2.

Metals may be burned by the electric fluid

Can we now question the burning of metals interiorly by the passage of the electric fluid, which some philosophers have long ago considered as endued with acid properties?

in vacuo.

Let my experiments be repeated in vacuo by a simple stream of the fluid, and it will be found, that the oxided and fuliginous products will be equal to those obtained in atmospheric air.

Effects of the experiment in water.

Let the experiment afterward be made in water, with short wires in a small tube, and very long ones in another tube; it will be found perhaps (but this requires farther examination) that the oxide precipitated at the place where a large wire comes out, as those of small tubes of silver, or of an alloy of gold and silver, will be less in quantity than in the tube four or five feet long, the positive wire of which is three fourths the length of the tube, and the negative wire one fourth. My friends form the same judgment in this respect. The oxided product was so copious, after the continued action of the electric current for five or six hours, that it covered the bottom to the extent of more than ten lines toward the apparatus; the black matter,

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struction of philosophical instruments, to saw the cylinder as the author requested. An interior view of the cylinder, when thus sawn asunder, is given at fig. 2. All the part, from the outer crack *a* to the centre, and a little beyond it, is torn; and in several places exhibits the same appearances as a broken iron bar, in others laminæ with a resemblance of crystallization, unless you would rather consider them as the effect of iron of a bad quality. The lower part of the cylinder, from the bottom of the hole *n*, is equally torn. A portion of the torn part appears to be oxided. The small detached pieces appear also to be oxided.

the cylinder.

The author has since sent me three other cylindrical pieces, one of which has an opening of six lines. They have all been burst by repeated electrical discharges, but I have not sawn them asunder. *J. C. Delamétherie.*

Others burst in the same manner.

which is a mixture of carbon and hidrogen, rendered the negative wire black for more than two inches; the water and the rest of the wire were tinged yellow; and when I made the discharge, this tinge spread more than a foot round the long wire, now become negative. All the rest of the water remained limpid.

Theory of two fluids.

When I first began these researches, I attempted to explain the phenomena of electricity by Symmer's theory of two fluids, because this theory is in no respect inconsistent with elective attraction, to which I have ever considered all substances obedient. But the moment I succeeded in detecting the passage of the fluid through the pores of uncoated glass, I confessed my mistake. I am ready to do the same, with respect to the inferences I draw from my last experiments, and I shall hereafter follow your advice, "seek for new facts, and let theories spring from them as they may."

Was not Mr. Orsted right in saying, that, when the electric fluid was better known, we should be able to explain several natural phenomena, hitherto inexplicable?

V.

Observations on the preceding Experiments. By J. C. Delametherie.

The four new planets fragments of a large one.

Do not these effects of electricity, in rupturing masses of so much tenacity as iron cylinders, give some probability to the idea of those German astronomers, who have said, that the four new planets, Ceres, Juno, Pallas, and Vesta, are fragments of a larger planet formerly situate between Mars and Jupiter, and broken by some unknown cause? Suppose, for instance, that the centre of this planet was a mass of metal, similarly circumstanced with the author's cylinders; and that a metallic vein, or any other conducting substance, acted like the leaden wire, and conducted the electricity of the atmosphere into the metallic mass: might not a great number of strong discharges, such as occur in violent thunderstorms, burst this metallic mass asunder, and project the different parts to a distance, as the little bit of lead in the author's valuable experiments?

Thunderstorms must act on the substance of our globe.

At least there can be no doubt, that these explosions, in our thunderstorms, must produce effects more or less considerable in the heart of our globe, as I have shown in my Theory of the Earth, vol. III, p. 224.

Theory of the Action of electricity on the Earth.

The terraqueous globe, I there observed, is commonly considered as a vast storehouse of the electric fluid. Natural philosophers call it the common reservoir. This supposes all the bodies, that compose the globe, to be in a state of habitual electricity; a principle admitted by all philosophers.

But this electricity is not always the same, either with respect to the whole body of the globe, or to its different parts.

It is completely demonstrated, that there is a reciprocal communication between the electricity of the atmosphere, and that of the globe. We must therefore consider the globe and its atmosphere as two electrical bodies, each charged with its natural electricity: and as these two bodies are in contact, their electricity should be in equilibrium according to their natural capacity, so that one has not more electricity than the other.

But local circumstances may increase the electricity of one of these bodies in certain places, and diminish it in others. The equilibrium will then be disturbed, and the electric fluid will rush from the positive body to the negative. This happens in the case of lightning, whether the stroke be descending or ascending.

When a portion of the atmosphere is positively electrified with respect to the terrestrial bodies opposite to it, the electric fluid rushes into these bodies, as soon as the distance allows. This communication is effected rapidly and with explosion, in certain circumstances; which constitutes the descending thunderstroke. On the contrary it is effected slowly and insensibly, if there be points to draw of the electric fluid gradually, or if it be conveyed by rain, dew, &c.

If the atmosphere be negatively electrified with respect to the opposite terrestrial bodies, it will attract their electric fluid. This will constitute the ascending stroke, if it be effected with explosion. Otherwise the communication will take place slowly and gradually.

Let us suppose, that the portion of the atmosphere over a lofty peak, as Mount Blanc, be electrified positively with respect to it; that is, contains more of the electric fluid: it will communicate its electricity to the mountain, either by flashes of lightning*, or slowly and gradually, and this electricity will pass from this lofty peak to the adjacent parts.

Let us suppose on the contrary a mountain like Etna, the vapours of which, ascending constantly from its internal fires, are loaded with positive electricity, which they take from the bosom of the mountain: the body of this peak must be in an habitual state of negative electricity, with respect to the parts of the earth contiguous to it.

VI.

Arrangement of the Strata of the Hill of Durbuy, in the Department of the Sambre and Meuse: *by* J. J. OMALIUS DE HALLOY†.

Arrangement of strata important in geology.

THE examination of the different arrangement of the strata that constitute the globe is one of the most interesting points of geology. Indeed till the existence of strata was suspected, this science did not begin to free itself from the chimerical forms, in which it was retained by absurd hypotheses framed on metaphysical abstractions, and take its place among the physical sciences founded on observation. The great book of Nature cannot be too frequently consulted; but as the ablest men have it not in their power to turn over all its pages, part of the task must devolve to subordinate labourers, who note down every thing that appears to them worthy notice, at the hazard of busying themselves about things of little importance.

Precipitates arrange them.

In the present state of things, every substance precipitated from a fluid in successive periods will arrange itself in horizontal strata: and what we know of the laws of gravitation does not allow us to conceive the possibility of a precipitate, the particles of which are not yet held together by the force of cohesion, supporting itſelf in vertical, or even inclined strata. Yet this position exists in many portions of the Earth, particularly those of a certain age.

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* Stones vitrified by flashes of lightning are found on Mount Blanc.

† Journal des Mines, vol. XXI, p. 475.

selves horizontally.

Strata not so.

It was natural, that various hypotheses should be formed, to account for such a singular fact. The opinion that appears best founded, and most generally adopted, is that of the sinking in of certain parts of the Earth. I do not mean to controvert an hypothesis adopted by many learned men, but I shall submit to them a circumstance, which I cannot explain.

Some in different parts of France.

The Ourthe, which flows through the department of that name, and that of the Sambre and Meuse, traverses a narrow valley, bordered by lofty hills, several of which are perpendicular. The mineral strata that compose these hills are all more or less inclined, and sometimes in a direction different from that of the valley. This takes place particularly at Durbuy, a small town in the third circle of the department, where the hill is formed of a blueish bituminiferous carbonate of lime, that constitutes several systems of strata. One of these systems is cut by a perpendicular plane, so that the remarkable arrangement of its strata is easily seen. See Plate VII, fig. 4. They have an inclination of about 60° or 80°, and are placed one upon another, like a series of pairs of rafters laid one upon another. The visible part of the first stratum in the centre exhibits only the shape of a wedge. On the top and sides of this stratum is placed a second, the summit of which is equally cuneiform; while the sides, sloping like those of a roof, cover the first wedge. Thus they continue in succession to the top of the hill. But the hill has been flattened by some cause or other, and the last strata have not the same summit as the rest, as they are merely applied on each side of the preceding, sloping in different directions, but not united at the top*.

Remarkable one.

What

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* It is very probable, that, if the upper surface of the hill were examined attentively, the exterior strata would be found to unite in their prolongation,

The strata thickest where they might have been expected to be thinnest.

What appears to me worthy of attention is the solid cuneiform summit, that unites two inclining sides, which are near 100 met. [109 yards] high from the river, and we know not how deep they descend. The particles that compose this summit are as intimately united as those of the rest of the stratum: there is no perceptible joint, no regular fissure, indicating the strain experienced by these strata in bending, if they were originally deposited in a horizontal situation: and supposing the calcareous matter to have been sufficiently soft when this happened, to bend without cracking, I cannot conceive what cause could produce a nearly pointed summit, resembling rafters united by a sloping cut, the obvious effect of which is an increase of thickness, while a simple bend necessarily diminishes it. Can the different velocities of the masses, combined with the pressure of the adjacent parts, which so happily account for strata bent on a small scale, apply equally to an entire hill, bent in the great, if the expression may be allowed, and the summit of which is not overtopped by other mountains *?

Geology of the neighbouring country.

These observations lead me to say a few words on the geological constitution of that portion of the departments of the Sambre and Meuse and of the Ourthe, which is included between the Meuse, the Lesse, and the Ourthe. The strata, that form the soil of this country, exhibit every possible variety of inclination. They not only vary from horizontal to perpendicular, but we every instant meet with curvatures or folds. Every thing indicates prompt and violent catastrophes. The direction † of these strata too

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prolongation, like those they cover; or that they would diverge, and the angular bend that unites them disappear at some distance from the precipice: for we can scarcely suppose, that the surface terminating all these strata at the top of the hill is exactly parallel to the line of junction of the central strata, and that it retains this parallelism throughout their whole extent. *Note of the French editors.*

* For some other singular arrangements of strata, see Journal, vol. XX, pages 62 and 64, and Plate II.

† I make a distinction between the direction and inclination of the strata: the latter is indicated by the angle, which the horizon forms with the lower plane of the stratum, while the direction is the common section of this plane with that of the horizon.

presents much irregularity in its minuter parts. But amid so much confusion we observe a pretty constant direction from south-west by south to north-east by north, making an angle of about 35° with the meridian. This situation of the strata coincides with the aspect of the country, divided into long hills, and narrow valleys, lying in the same direction. But these valleys are not the only ones, that furrow the country; for the hills are frequently intersected by irregular valleys, commonly serving as a passage for the rivers.

Inferences.

Does not this agreement of the longitudinal valleys with the direction of the strata lead to the supposition, that their origin is connected with the circumstances that formed the strata, or gave them their inclination? and the irregularity of the tranverse valleys, and their agreement with the present course of the waters, that they are owing to subsequent events, produced by the course of some fluid?

I shall conclude with an observation, the explanation of which appears also to depend on hypotheses relative to the causes of the inclination.

Alternation of gritstone, schist, and limestone.

All the strata of this country are composed in general of gritstone, schists, and bitumeniferous carbonate of lime, the reciprocal superposition of which announces a contemporary formation[*]. Yet we may observe in an infinite number of instances, that the plains, or summits of the hills, exhibit only gritstone or schist, while the declivities present these strata alternating with limestone; and that the latter substance alone forms the bottom of most of the longitudinal valleys[†]. I do not think this effect can be ascribed to erosion by water; for in the transverse valleys the limestone appears most unalterable. While the acclivities of schist and gritstone are covered with vegetation, those of limestone exhibit perpendicular sections and sharp ridges.

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[*] All these are of secondary formation, and contain fragments of organized substances, such as vegetables, and animals without vertebræ.

Pebbles.

[†] In these valleys are found many blackish pebbles, which I consider as real quartz-agates (silex), possessing all the outer characters of the *kiesel-schiefer* of the German mineralogists. This substance indeed, like all the quartzes, is infusible, and Wiedenmann speaks of the fusibility of the kiesel-schiefer. But may not this property be ascribed to the accidental admixture of some calcareous particles? an opinion the more probable, as our black quartzes are frequently found in nodules in carbonate of lime.

VII.

Method of stabbing Hoven Cattle, to discharge the Rarified Air from the Stomach, when they have been overfed with moist Clover Grass: communicated by Mr. W. Wallis Mason, *of Goodrest Lodge, near Warwick*[*].

Gentlemen,

Instrument for relieving hoven cattle.

I Beg leave to lay before you a trocar and canula, for the relief of cattle, when gorged or hoven. Since I have introduced it, it has been used with the greatest success, having, in every instance tried, been proved a safe, easy, and effectual remedy.

Many annually lost by this affection.

I consider it will not be necessary for me to detail the dangerous consequences arising from cattle being hoven, as it is well known, that the public are annually deprived of numbers of valuable cattle by this disorder. I am inclined to offer it as an instrument superior to that, for which the Society granted a premium in the year 1796; as I am of opinion, that flexible tubes may be forced down the passage, which conducts to the lungs, by which most dangerous consequences would ensue. An instance of this kind occurred last year in this neighbourhood, when intending to force the passage of the paunch, and occasioned the loss of the animal.

Inconveniences of a former instrument.

Neither the farmer nor bailiff can be expected when going the rounds of the farm, to carry with him at all times an instrument so large as one of the flexible tubes; even if he had it, he could not make use of it without the assistance of a second person, and the disorder would be fatal in most instances, before such assistance could be procured.

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[*] Trans. of the Soc. of Arts, vol XXVI, p. 128. The silver medal was voted to Mr. Mason for this communication.

I confidered,

I considered, that the trocar and canula commonly used by surgeons might be employed to advantage for the relief of hoven cattle. I have improved the instrument, to answer better the purpose here intended of penetrating the tense hides of cattle; and such alteration materially facilitates the operation.

Trocar and canula preferble.

The method of applying it is, to penetrate with the trocar and canula through the hide of the beast to the paunch on the near side, about six inches from the back-bone, at an equal distance from the last rib and from the hip-bone; then to withdraw the trocar, and to leave the canula in the wound, until the air which the paunch contained has escaped. The canula may then be taken out, and the wound covered with a plaster of common pitch, spread on brown paper, about the size of a crown piece. All the danger incidental to the common mode of stabbing with the knife is effectually prevented, by the canula being left in the incision when the trocar is withdrawn.

Method of using them.

The small expense of the instrument, its portability, the ease with which it can be used by an individual, its safety and efficacy in use, as it has not in any instance failed of complete success, will, I hope, be sufficiently evident, to recommend it to the attention of the Society. A great saving would arise to the owners of cattle, and to the country at large, from general adoption of its use.

Their advantages.

A feeding ox will thrive as well after the operation, as if it had never been affected by the disorder. Cows in calf are in no danger from its use. It has been found particularly beneficial in preserving rearing calves, and young cattle, when afflicted with this disorder, which had heretofore been fatal to great numbers of them.

I beg leave to add the certificates of a few of those gentlemen who have witnessed the utility of this method, and whose recommendations have stimulated me to submit it to the Society, in hopes, that by their liberal patronage it may be rendered more generally beneficial to the public.

I have the honour to remain, Gentlemen,

Your obedient servant.

W. WALLIS MASON.

Certificates.

Certificates from the following gentlemen testified, that they had experienced the efficacy of Mr. Mason's trocar, had proved the safety of the operation, and the instantaneous relief which it had never failed to produce without leaving any blemish, or dangerous consequence, from its application.

JOHN FORD NAISH, Leek Wooton.
THOMAS BRYAN, Warwick.
WILLIAM ORAM, Warwick.
WILLIAM LEDBROOKE, Northend.
RICHARD CATTELL, Milverton.

Reference to the Engraving of Mr. WALLIS MASON'S *Trocar and Canula.*

Instrument described.

Fig, 1, Pl. VIII, is a representation of Mr. Wallis Mason's trocar and canula. The blade of the trocar is of steel *a a*, fig. 4, fixed into the wooden handle *b b*. The shape of the blade of the trocar is oval, as shown in the end view of the canula, fig. 3. The canula or sheath *c c*, figs. 1 and 2, is an oval tube, which exactly fits the blade of the trocar; *f f* is a concave circular plate, fixed at the end of the canula, forming a hilt, to prevent the instrument from giving too deep a wound when used; the end *g* of the canula is worked down to a sharp edge, that it may not obstruct the passage of the instrument. The drawings are on a scale of one inch to two inches and a half; in figs. 1 and 2, the trocar and canula are shown edgeways, or in the shortest diameter of the ellipsis; in fig. 4, the trocar is shown flat, in its longest diameter.

VIII.

Description of a Swivel-headed Churn Staff, to facilitate the making of butter: by Mr. TIMOTHY FISHER, *of Ormskirk, Lancashire*[*].

SIR,

Swivelheaded churnstaff.

I Beg leave to lay before the Society of Arts &c. my swivel-headed churn staff, which, on repeated trials, is now fully proved to answer the very desirable purpose of relieving the hard labour of churning; which it does in a much greater degree than could be supposed, from a slight view of its simplicity and apparent small deviation from the common churn staff. It however passes with much more ease through the cream. It must be worked much slower than the common, otherwise it is found to churn the cream too soon, or, according to the technical term in this county, *to swelt it*.

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[*] Trans. of the Soc. of Arts, vol. XXVI, p. 131. Five guineas were voted to Mr. Fisher for this invention.

Require to be worked slowly.

I have tried it in a variety of forms and sizes; with six wings the labour was less relieved; also when I gave less bevel to the ends of the wings.

When I gave more bevel it passed through without producing the intended effects. Experience therefore has convinced me, that it is best to have four wings from six to seven inches in length, from the centre, according to the size of the churn for which it is intended, from two and a half to three inches in breadth, made plane in the centre or middle, about the fourth part of their length, and then bevelled regularly off, so that the ~~extreme point shall form~~ an angle of about 45 degrees with the plane of the middle. The plane part acts with its usual force upon the middle of the body of the milk; and the points turning rapidly round give a kind of compound motion to the whole, and that also alternate, and yet it does not in the least splash or throw out the cream as in the common mode.

Best form of it.

I am, Sir,

Your obedient servant,

TIMOTHY FISHER,

Gun-maker.

Thomas Ecclestone, Esq., of Scarisbrick Hall, near Ormskirk, certified, that he had seen Mr. Fisher's new method of churning butter, and that he thinks it superior to any he had heretofore known for that purpose, and that such was also the opinion of several other persons in the farming line who had witnessed its effects.

Its superiority.

Reference to the Engraving of Mr. FISHER'S *Churn Staff.*

Fig. 7, Pl. VIII, is a section of an upright churn, in the situation it would be when at work, and figs. 6 and 6 are enlarged

Explanation of the plate.

larged views of the head of the churn staff. ABDE fig. 7, is a section of the churn; FG is the lid; KL is the churn staff, and HI the wings, or beaters; it is this part only which differs from the ordinary churn; it consists of four wings or vanes MNOP, fig. 5, firmly fixed together, and turning freely on a pin driven into the end of the churn staff. The flat part of each vane is cut, so as to be inclined to the plane in which all four lie, in the same manner as the sails of a windmill, as is well explained by figs. 5 and 6. When the beater is moved up and down through the cream, its action upon the oblique vanes causes it to turn round upon a pin above mentioned, as a centre. *a*, Fig. 7, is a small wooden bolt sliding in a groove made in the churn staff, KL; its end shuts into a hole *b*, fig. 5, made in one of the vanes; when this bolt is puſhed down, it prevents the vanes from turning round, for the purpose of collecting the butter together at the top of the butter-milk when the churning is done.

IX.

Improvement in Cutting Silk Shag Edgings: by Mr. PETER TANSLEY, *at the White Horse, Wheeler Street, Spitalfields**.

SIR,

Narrow shag tedious to cut with the common implement.

IN the latter end of March, 1806, my employer, Mr. Jeffery of Bow Lane, Cheapside, applied to me with a pattern of silk shag, about six eighths of an inch wide, which he wished to be executed as quick as possible.

Being aware how tedious an operation it was to cut the shag with the trivat, the instrument generally used by the shag weavers for that purpose, I thought I could invent an instrument, which would cut it in one third of the usual time; I therefore drew the plan of one, and gave it to Mr.

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* Trans. of the Soc. of Arts, vol. XXVI, p. 164. Five guineas were voted to Mr. Tansley for this invention.

Another therefore invented.

Baker, a cutler in Artillery Passage, Artillery Street, Spitalfields, to make according to my instructions, which he did, and which answered my expectations in the work.

In October, 1806, I made for the same gentleman a figured shag, one inch and three quarters wide, and cut it with the same instrument, as certified by my employer and his son in law. I hope the Gentlemen of the Society will not think me too presuming in offering this improvement to their notice.

I am, Sir, with great respect,
Your obedient Servant,
PETER TANSLEY.

Reference to the Engraving of Mr. PETER TANSLEY'S *Implement for Cutting Shag. See Plate VIII, Figs.* 8. 9.

Fig. 8 shows the steel implement or cutter, as placed or woven within the threads, previous to their being cut with it, the cutting being performed by merely drawing out the knife, the sharp edge of which cuts the threads, and forms the shag by that operation.

Fig. 9 shows the implement detached from the work, a being the part held in the hand, and s the sharp blade which cuts the threads.

SIR,

This is to certify, that Peter Tansley, of Wheeler Street, is the sole inventor of the cutter for making narrow shag; that its utility is well known; and that I have, within fourteen days, made four hundred of these improved instruments.

W. BAKER.

Certificates were received from several other persons, stating the very great utility of the instrument, their persuasion of its being superior to any other thing of the kind ever invented, as making the work equally good, with greater ease and in half the time, and that Mr. Peter Transley is the original inventor.

IX.

X.

Observations on the Combustion of several Sorts of Charcoal, and on Hidrogen Gas: by THEODORE DE SAUSSURE.

(Concluded from p. 176.)

Combustion of plumbago from Cornwall.

lumbago from ornwall, containing 0·04 of on, burned in rigen.

I Burned in oxigen gas 0·588 of a gramme [9·079 grs.] of plumbago dried at a red heat. The combustion continued an hour, and left as a residuum 0·033 of a gr. [0·51 of a gr.] of red oxide of iron. This compound, which is formed during the operation *, would consist, according to Bucholz †, of 0·0231 of a gr. [0·357 of a gr.] of iron, and 0·0099 of a gr. [0·153 of a gr.] of oxigen. One hundred parts of this plumbago therefore contain 4 parts of iron; and I burned 0·588 — 0·0231 = 0·5649 of a gr. [8·722 grs.] of carbon.

esults.

The gas contained in the receiver occupied in the shade, previous to the combustion 1894·3 cent. cub. [1153·5 cub. inch.]; thermom. at 23·12° [73·62° F.]; barometer, reduced to the temp. of 12·5° [54·5° F.], 0·7329 of a met. [28·83 inch.].

Two hours after the combustion the gas occupied 1899·3 cent. cub. [1156·57 cub. inch.]; thermom. at 25° [77° F.]; barom. corrected at 0·7329 of a met. [28·83 inch.].

Reducing the volumes of gas, before and after combustion, to the mean temperature of 12·5° [54·5° F.], and pressure 0·75796 of a met. [29·82 inch.], we find that the gas occupied,

Before combustion, 1758·8 cent. cub. = 1071·022 cub. inch.
After combustion, 1750·4 = 1065·907
Diminution, ······ 8·4 = 5·115

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* Messrs. Allen and Pepys, in making the same experiment, consider the oxide of iron as completely formed in the plumbago before the combustion. This must occasion some slight difference in the results of their operation.

† Ann. de Chim. vol. LXV, p. 202: or Journal, vol. XXV, p. 353.

The

No smoke, or vapour, emitted;

The plumbago emitted neither vapour nor smoke in burning. The muriate of lime placed in the receiver, and which was always weighed in a closed phial, acquired an increase of 5 cent. [0·772 of a gr.]: but I learned from a comparative experiment, that it absorbed a centigramme [0·154 of a gr.] of water from the atmospheric air during the time of its being put into the receiver and taken out again. The gas employed for the combustion contained no visible water, but it was in a state of extreme humidity; and the hygrometrical water in it, at the temperature of 25° [77° F.], weighed 3·9 cent. [0·6 of a gr.]. As the muriate of lime must have acquired 4·9 cent. [0·754 of a gr.] by these two additions of weight, it does not appear, that the plumbago produced any sensible quantity of water in burning.

and no water produced.

State of the gas employed.

The hidrosulphuret of potash indicated 189·75 parts of oxigen gas, and 10·25 of nitrogen, in 200 of the impure oxigen gas, which the receiver contained before the combustion. Potash detected no acid gas in it. 200 other parts of the same gas, mixed with 400 of hidrogen gas, were reduced by detonation to 33; and consequently contained 189 oxigen and 11 nitrogen.

Carbonic acid formed.

From 100 parts of the gas left after the combustion of the plumbago potash absorbed 63·42 of carbonic acid gas.

No hidrogen evolved.

After the separation of this acid gas, I examined whether there were any hidrogen gas in the residuum, by detonating it with a mixture of equal parts of hidrogen and oxigen, and treating with potash the gas remaining after the detonation. These operations showed me, that the plumbago had given out no hidrogen.

The hidrosulphuret of potash indicated in 100 parts of the gas, which the receiver contained after the combustion of the plumbago [and abstraction of the carbonic acid], 87 parts of oxigen gas, and 13 parts of nitrogen. Another 100 parts were mixed with 200 of hidrogen gas, and reduced to 40 by detonation. These 100 parts therefore contained 86·66 of oxigen, and 13·34 of nitrogen. According to the process with hidrosulphuret, the 1750·4 cub. cent. of gas found in the receiver after the combustion of the plumbago contained

State of the gas after the process.

Carbonic acid gas	1110·1 cent. cub.	676 cub. inch.
Oxigen gas	557.06	339·22
Nitrogen gas	83·24	50·68
	1750·4	1065·9

Composition of carbonic acid gas.

If we calculate the composition of the carbonic acid gas from the quantity of oxigen employed to form it, we find, that the oxigen gas which disappeared in this process was 1669·8 — 557·1 = 1112·7 cent. cub. And deducting 7·3 cent. for the weight of 0·0099 of a gr. of oxigen, that oxided the iron in the plumbago, there will remain 1105·4 cent. cub. [673·13 cub. in.] of oxigen gas, that entered into the composition of 1110·1 cent. cub. [676 cub. in.] of carbonic acid gas. If instead of the measures of these gasses we substitute their respective weights, we find by the rule of proportion, that 100 parts of carbonic acid gas by weight contain 72·64 of oxigen, and 27·36 of carbon.

100 parts contain 27·36

If we calculate the composition of the acid gas from the weight of the plumbago burned, we find, that 0·5649 of a gr. [8·722 grs.] of plumbago (deducting the iron contained in it) were employed to form 1110·1 cent. cub. [676 cub. in.], or 2·0621 gr. [31·839 grs.] of carbonic acid gas. Consequently 100 parts of this acid by weight contain 27·39 parts of carbon, and 72·61 of oxigen.

or 27·39 of carbon.

2d Experiment on the Combustion of Plumbago from Cornwall.

2d Exp. on plumbago.

As plumbago was the only carbonaceous substance, of all I tried, that yielded neither water nor hidrogen in burning, I thought it necessary, to repeat the process.

Results.

27·38 or 27·04 per cent of carbon.

The results were nearly the same as before. The composition of the carbonic acid gas came out 27·381 carbon, and 72·619 oxigen, calculating from the weight of plumbago burned; and 27·04 of carbon, 72·96 of oxigen, calculating from the oxigen gas consumed.

Diminution of the gas.

Two other trials were made, and in all there was a little diminution of the oxigen by burning, owing chiefly to the oxidation of the iron mixed with the plumbago.

Combustion

Combustion of Anthracite.

Stone coal burned.

I burned in oxigen gas 0·549 of a gr. [8·476 grs.] of anthracite dried at a red heat*. The combustible that disappeared in this operation weighed 0·524 of a gr. [8·09 grs.], deducting the ashes, which weighed 0·025 of a gr. [0·368 of a gr.]

The receiver, in which the combustion was effected, contained previous to it 1886·4 cent. cub. [1148·7 cub. in.] of impure oxigen gas, at 0·73089 met. [28·755 in.] of the barom., and 21·56° of the therm. [70·8° F.]

After the combustion the gas occupied the space of 1894·8 cent. cub. [1153·5 cub. in.], at the same pressure, but at the temperature of 24° [75·2°].

Reducing these quantities of gas to the mean temperature of 12·5° [54·5° F.], and pressure 0·75796 met. [29·82 inches], we find, that the volume of gas was

Diminution of the gas.

Before the combustion ··	1755 cent. cub.	=	1068·7 cub. in.
After the combustion ··	1748·2	=	1064·56
Diminution ··········	6·8	=	4·14

Smoke.

The anthracite formed a little smoke in burning. The muriate of lime increased in weight 8 cent. [1·235 gr.]; but as the hygrometrical water of the oxigen gas weighed 0·0336 of a gr. [0·519 of a gr.] at the temperature of 22° [71·6° F.], at which the process was conducted; and as the salt absorbed 1 cent. [0·154 of a gr.] of water from the air, during the time of arranging the apparatus; I found, that the quantity of water produced by the combustion of the coal was about 0·0364 of a gr. [0·562 of a gr.]

Water formed.

State of the gas before the combustion.

In 100 parts of the gas employed for the combustion the hidrosulphuret of potash indicated 94 of oxigen and 6 of nitrogen; Volta's eudiometer, 94·25 of oxigen, and 5·75 of nitrogen. Hence the 1755 cent. cub. of gas employed in the combustion contained 100·9 of nitrogen.

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* This anthracite is found in nodules in a transition pudding-stone, near Martigny, in Vallais. It loses by incandescence 0·16 of its weight, and then attracts a little moisture from the air. For this reason I did not leave it exposed to the air after drying. On cooling it absorbed its own bulk of atmospheric air. This absorption may be neglected.

State after it.

The 1748·2 cent. cub. of gas in the receiver after the combustion were found to consist of

Carbonic acid gas	998·27 cent. cub.	= 607·9 cub. inch.
Oxigen gas	616·82	= 375·61
Nitrogen gas	100·9	= 61·44
Oxicarburetted hidrog.	32·21	= 19·61
	1748·20	= 1064·56

Combustion of the Coal formed by Oil of Rosemary.

Charcoal from essential oil.

By passing oil of rosemary in a state of vapour through a red hot porcelain tube, I obtained a coal, which appeared well adapted for determining the composition of carbonic acid.

Its properties.

The coal, heated red hot in the fire, and extinguished without the contact of air, did not absorb different gasses, in which it was immersed. It did not increase in weight after this incandescence, even though exposed several months to the open air. It formed no ashes. It burned with the same difficulty as plumbago. It was heavy, and sufficiently hard to scratch glass.

Burned.

I burned 0·513 of a gr. [7·92 grs.] of this coal in a receiver containing 1947·93 cent. cub. [1186·19 cub. in.] of oxigen gas at 26·87° [80·37° F.] temperature, and 0·73684 met. [28·99 inches] pressure.

Results.

After the combustion the gas occupied the space of 1958·83 cent. cub. [1192·83 cub. in.], at 27·5° [81·5° F.] temperature, and pressure as before.

On reducing the volume of the gas before and after the experiment to the mean temperature of 12·5° [54·5° F.], and pressure of 0·758 met. [29·82 in.], we find it occupied

Increase of the gas.

Before combustion,	1791·5 cent. cub.	= 1090·93 cub. in.
After	1797·2	= 1094·4
Increase	5·7	= 3·47

No visible vapour or smoke was formed during the process. The muriate of lime suspended in the receiver acquired an addition of 5 cent. [0·772 of a gr.]; but I have already observed, that this would result from the water contained in the oxigen gas, and in the atmospheric air to which it was a few moments exposed.

No smoke emitted.

No water formed.

The gas in the receiver after the combustion consisted of

State of the gas.

Carbonic acid gas····	1008·9 cent. cub.	= 614·37
Oxigen gas ········	508·5	= 309·65
Nitrogen, and oxicarburetted hidrogen*	279·8	= 170·38
	1797 2	= 1094·4

On calculating the composition of the carbonic acid gas from the weight of coal burned, it comes out 27·109 of carbon and 72·891 of oxigen; and from the oxigen consumed, 72·88 oxigen and 27·12 carbon.

100 parts carbonic acid contain 27·109 or 27·12 of base.

Combustion of Box Charcoal.

I exposed some charcoal, made from box, fastened on a plate of platina by wires of the same metal, to a long-continued red heat in a close vessel. This charcoal was weighed in a close vessel immediately after cooling, and was not afterward exposed to the air longer than was necessary for putting it into the receiver, where it was to be burned. When this combustible in a state of incandescence was immersed in mercury, cooled there, and passed through it into a jar full of common air, this air underwent in a few instants a diminution equal to seven times the bulk of the charcoal. In the present experiment this charcoal weighed 0·591 of a gr. [9·125 grs.], displaced 0·95 of a cent. cub. [0·579 of a cub. in.], and consequently contained 6·65 cent. cub. [4·05 cub. in.] of atmospheric air. After combustion in oxigen gas it left a residuum of 2 cent. [0·31 of a gr.]

Box charcoal burned.

Absorbs 7 times its bulk of air presently.

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* The quantity of oxicarburetted hidrogen must have been but very small, since the bulk of the nitrogen present before the combustion was 275 cent. cub. [167·46 cub. in.]

Increase of gas.

The gas reduced to mean temperature and pressure measured

Before the combustion,	1683·7 cent. cub.	=	1025·29 cub. in.
After	1709·7	=	1041·12
Increase	26		15·83

Vapour emitted.

Water produced.

The charcoal formed a light vapour in burning. The increase of weight of the muriate of lime in the receiver, after deducting that of the water absorbed from the oxigen gas and from the atmosphere, was 2 cent. [0·31 of a gr.], the weight of the water produced by the combustion of the charcoal.

State of the gas after combustion.

The gas in the receiver after the combustion consisted of

Carbonic acid gas	1074·4 cent. cub.	=	654·26 cub. in.
Oxigen gas	555·89	=	338·51
Nitrogen gas	60·08	=	36·58
Oxicarburetted hidrogen	19·33	=	11·77
	1709·7	=	1041·12

State before.

The receiver contained 56·4 cent. cub. [34·34 cub. in.] of nitrogen previous to the combustion, beside what belonged to the atmospheric air absorbed by the charcoal.

100 parts carbonic acid contain 27·15 of carbon.

On calculating the composition of the carbonic acid gas from the weight of the oxigen, that disappeared during the combustion, we find it to consist of 72·85 oxigen, and 27·15 carbon. The calculation from the weight of the charcoal consumed comes very near this; but as it requires a conjectural estimation of the weight and formation of the oxicarburetted hidrogen, it can be considered only as an approximation.

This charcoal contained both oxigen and hidrogen.

This experiment is not of itself sufficient to decide, whether both the elements of the water existed in the charcoal before the combustion, or the hidrogen alone; but it is to be observed, that the bulk of the carbonic acid produced was fully equal to that of the oxigen gas consumed; and, the result being the same as that furnished by the plumbago, which formed no water, we have hence reason to presume, that both the elements of this fluid existed in the charcoal.

Combustion

Combustion of Charcoal that had been employed for preparing liquid hidroguretted sulphur.

Charcoal left after the preparation of liquid sulphuretted hidrogen.

The great quantity of hidrogen gas that is produced, when sulphur is passed over charcoal in a red hot tube, has led to the conjecture, that this gas does not belong wholly to the sulphur; and that this process might afford the means of obtaining charcoal perfectly pure, or freed from hidrogen*. For this reason I examined the products of the combustion of some fir charcoal, which had lost half its weight in the formation of liquid sulphur; and which had been afterward exposed to a red heat in contact with air, till it appeared to be divested of sulphur, or no longer diffuse any sulphurous smell while incandescent. Immediately after cooling it was enclosed in a phial, and weighed in it. It then weighed 0·532 of a gr. The charcoal I burned absorbed in cooling 4½ times its bulk of atmospheric air, or 0·01 of a gr.; and this being deducted, 0·522 of a gr. [8·06 grs.] remained for the true weight of the charcoal. When burned in a receiver filled with oxigen gas, it left 0·012 of a gr. [0·185 of a gr.] of ashes.

Burned in oxigen gas.

Retains sulphur very strongly,

On taking out of the receiver a sufficient quantity of gas for examination, and throwing away the rest, contrary to my expectation it emitted a very striking smell of sulphurous acid gas, though the red hot charcoal had no perceptible smell. The extreme difficulty with which this combustible burns can be ascribed only to the intimate combination of the sulphur with the charcoal. Common charcoal acquires by long-continued incandescence a kind of incombustibility, but it does not come near that of sulphuretted charcoal.

which renders it difficult of combustion, much more than long incandescence.

The gas in which this charcoal was burned consisted of

State of the gas before combustion.

Oxigen gas	1537·9 cent. cub.	=936·51 cub. in.
Nitrogen	123·3	= 75·08
	1661·2	=1011·59
Measuring after combust.	1688·2	=1028·03
Increase	27	= 16·44

Increase.

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* Ann. de Chim. vol. LXI, p. 139: or Journal, vol. XVIII, p. 48.

State after combustion.

After the combustion the gas consisted of

Carbonic acid mixed with sulphurous acid gas	950·45 cent. cub.	= 578·78 cub. in.
Oxigen gas	595·06	= 362·36
Nitrogen	126·80	= 77·21
Oxicarburetted hidrogen	15·89	= 9·68
	1688·2	= 1028·03

Water produced.

From this combustion we cannot deduce any very precise conclusions respecting the composition of carbonic acid gas; since, independent of the uncertainties arising from the production of water, which amounted to 0·018 of a gr. [0·278 of a gr.] and of the oxicarburetted hidrogen, some sulphurous acid was produced, and absorbed by the potash with the carbonic acid. We may presume however, that the charcoal contained but a very small quantity of sulphur and hidrogen; for, if we were to consider the mixture of carbonic acid and sulphurous acid gas as so much pure carbonic acid gas, the results of the combustion of the sulphuretted and hidroguretted charcoal would approach very near those furnished by the carbon of plumbago.

Furnishes nearly as much hidrogen as common charcoal.

The results of this combustion however show, that the charcoal employed in the formation of hidroguretted sulphur furnishes nearly as much hidrogen, as charcoal that has not been subjected to this process. Hence we may infer, that the hidrogen of the hidroguretted sulphur is not furnished by the charcoal. It is probable, that sulphur contains either water, or oxigen and hidrogen; and that the charcoal forms the hidroguretted sulphur by abstracting the hidrogen.

General results

Recapitulation of the principal Observations.

Plumbago.

Cornish plumbago, burned in oxigen gas, yields nothing but carbonic acid gas and oxide of iron, without any mixture of water, or of hidrogen gas.

Its combustion shows, that 100 parts of plumbago contain 96 of carbon and 4 of iron; and that 100 parts by weight of carbonic acid gas contain between 27·04 and 27·38 parts of carbon. We find the same results, whether we calculate from the weight of the carbon burned, or the quantity of oxigen consumed, supposing the carbonic acid gas to contain its own bulk of oxigen gas.

Charcoal of essential oil.

The purest charcoal I have burned, next to plumbago, is that produced by decomposing the essential oil of rosemary in a red hot tube. Probably other oils of the like kind would furnish a similar charcoal. In its combustion it did not form any notable quantity of water: but it gave out some oxicarburetted hidrogen, though in too small a quantity, for the composition of the acid gas to be sensibly modified by it. From this experiment I found, that 100 parts of carbonic acid contain 27·11 of carbon, and 72·89 of oxigen.

Stone coal.

The combustion of anthracite, previously exposed to a red heat, furnished too perceptible a quantity of water and of hidrogen, for the results of this process to be calculated with accuracy, and compared with the preceding.

Box charcoal.

The combustion of box charcoal too, dried by long incandescence, furnished an appreciable quantity of water and oxicarburetted hidrogen.

Charcoal used for preparing liquid sulphuretted hidrogen,

The combustion of charcoal, that had served for the preparation of liquid hidroguretted sulphur, produced very nearly the same quantity of water and hidrogen, as was obtained from dried charcoal, that had not been exposed to the action of sulphur. Hence we may infer, that this substance does not take from charcoal the hidrogen it contains. It is probable, that sulphur contains either water or its elements; and that charcoal occasions the sulphur to become hidroguretted by attracting the oxigen. The recent experiments of Mr. Davy on sulphur by means of Voltaic electricity leave no doubt, that this substance contains a pretty considerable quantity of oxigen and hidrogen.

Sulphur contains oxigen & hidrogen.

Increase or diminution of the gas.

In these experiments where I burned charcoals containing hidrogen, the gas, in which the combustion was effected, was sometimes a little increased, at others a little diminished in bulk. This difference appeared to me, to depend chiefly on the more or less perfect combustion of the hidrogen evolved. This combustion was more or less complete, not only according to the proportion of circumambient oxigen gas, but to the intensity of the heat, which in my experiments varied.

XI.

On Meteorological Nomenclature, in answer to LUKE HOWARD, *Esq.* *By* J. BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR, *Liverpool, July* 11, 1810.

On meteorological nomenclature.

I Read with some concern, in your last number, the critique of Mr. Luke Howard on my meteorological observations. I was concerned to find, that my attempts to illustrate a part of science, upon which he has bestowed so much attention, should not have met with a more favourable reception from him; and I was still more concerned to perceive, that he was offended at me, for rejecting the nomenclature which he proposed, to express the different modifications of the clouds. I briefly explained my motives for so doing, and surely not in a way to have called for any harsh censure; but I shall now state more fully what I have done on the subject, in order that Mr. Howard may judge whether I was influenced by the desire of advancing my reputation at the expense of his, or even by the affectation of singularity.

I had long been in the habit of making accurate observations on meteorological phenomena, when Mr. Howard published his essay; and the first impression which it made upon me was a very favourable one, many of the terms appearing to be both characteristic and judicious. I regretted indeed, that they appeared to be connected with a peculiar hypothesis, to which I could not altogether assent; yet I determined to employ them in my own diary, and I made the experiment with both the desire, and the expectation of finding them materially useful to me. After giving them, however, a fair trial, I found them quite inadequate to express the different atmospherical appearances, which I thought of sufficient importance to be recorded, and, after some time, discontinued their use. Whether the defect in this case was imputable to the nomenclature itself, or to my unskilfulness, On Meteorological nomenclature.

unskilfulness, it is not for me to decide; but it was chiefly on this account, and partly from its connection with a peculiar hypothesis, that I neglected it, and not from any selfish desire "of making way for my own" in opposition to it.

Mr. Howard refers me to an article in Dr. Rees's Cyclopedia, where I may find his ideas on the subject, detached from the theory with which they were before united, and resting on observation alone. This essay I shall not fail to take the first opportunity of perusing, and I shall probably find, that it will remove part of my objections to the new nomenclature. With respect to the other objection, arising from its want of minuteness and comprehension, I think the only method of effectually answering it will be for Mr. Howard himself to give a specimen of a diary constructed as mine is, so as to afford a complete history of the atmospherical phenomena of each day, expressed in his own language. I feel confident, that Mr. Howard will agree with me in thinking, that such a series of observations will tend very much to improve the science of meteorology; and I have no doubt, that he will have sufficient candour to acknowledge, that the diary, which he formerly furnished for the Athenæum was by no means sufficiently copious for the purpose.

I am so desirous of carrying my project into execution, that I shall be happy to enter upon any plan of cooperation, that shall be suggested by any of your correspondents, and none more so than by Mr. Howard. So far from having any partiality for my peculiar phraseology, no one can be more aware of its defects than myself, or more desirous, that it should be exchanged for a language that may be more scientific, and more correct.

I am, Sir,

Your obedient servant,

J. BOSTOCK.

XII.

Method of increasing the Durability of Tiles: by Count VON MELLIN*.

Methods of rendering tiles more durable.

MEANS of increasing the hardness of tiles, and consequently their durability, have been sought, without the discovering of any sufficiently cheap and simple for common use. Such is the glazing or varnishing of tiles, which is indeed very good, but too expensive, to be generally adopted. Tarring tiles has been proposed; and this process appearing to me easily practicable and not expensive, I determined to make trial of it on one of my roofs, that required a great deal of repairing. Providing some of the largest brushes I could get, I and an assistant set ourselves to coat the upper side of my tiles with tar liquefied over a gentle fire, and kept moderately hot. Four persons were employed to hand us the tiles, and, when tarred, to lay them in the sun to dry; which took three or four days, it being then the spring of the year. It is proper to say, that I had set apart the best tiles, or those which appeared most thoroughly baked; and that I exposed the others to the sun, that they might be warmed and receive the coat of tar more easily. After the process these appeared as if coated with a reddish brown varnish. Four hours were sufficient for the preparation of two thousand.

Glazing expensive.

Coating with tar cheap and effectual.

Method employed.

Another method.

Near my house was a tile kiln, which was just ready to draw. As soon as it was sufficiently cool, to allow the tiles to be handled, I had as many taken out, as left in the interior of the kiln sufficient room for a few people to coat them with tar. While two of these were tarring the tiles, three others were employed to give them, receive them when tarred, and lay them in a corner of the kiln, where the heat was reduced to that of a vapour bath. When the kiln was quite cold, the tiles were perfectly dried, but they had not such a shining coat as the former, because the great heat had caused the tar to penetrate into their substance. Their pores were completely stopped, and they were rendered impenetrable to water, as I found by experience. The five persons I have mentioned tarred four thousand tiles in six hours. Both these experiments did not consume a barrel of tar.

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* Sonnini's Biblioth. physico-écon. Oct. 1808, p. 243.

The tiles not injured in 29 years.

The roof for which these tiles were used is open to the north, and exposed to all the violence of winds and storms. It was repaired in 1779, and not one of the tarred tiles is at all injured or decayed. They are covered with a very fine mass, and their surface is in as good condition, as if the tar had just been laid on. On the other hand, several of those which, as I said before, I had set apart, supposing they would resist the weather without any preparation, because they were thoroughly burned, are cracked, broken at the corners, or splintered on the surface.

Additions to the tar injurious.

Some persons say, that tarred tiles would be more durable, if they were powdered with iron filings and charcoal dust: but I conceive these substances would render the surface rough; and thus detain the water, while those coated with varnish would let it run off.

Other substances.

I am of opinion however, that a mixture of lime and tar would be more beneficial. I think too, that fats in general, whale oil, or the dregs of our oils, would be equally adapted to the purpose, and still cheaper.

ANNOTATION.

Calculation of the expense in this country.

Few people in London or its vicinity, where tiles are the common covering of houses, but must have experienced great inconvenience from roofs leaking, and the consequent trouble and expense of frequent repairs. Sometimes indeed this is owing to the badness of the mortar employed; but is most commonly the consequence of a few tiles being cracked to pieces by frost, after they had imbibed water. The method above recommended would appear to be a sufficient remedy for this; and the expense attending it is not an object at all comparable with the comfort and advantage of a secure roof. I am not certain, whether the count be speaking of plain tiles, or pantiles; but taking them to be plain tiles, the least favourable supposition, and the size of ours, a roof of 24 feet by 25, which would be that of a house of middling size would take about 4000. Now two thirds of a barrel of tar, at £2 6s. a barrel, the highest price in the market at the present time, come to £1 10s. 8d; and the labour, at the rate of 6 men for 8 hours, the longest time in the two experiments above, at 5s. a day, will be £1 4s; so that the whole additional cost of a moderate sized roof would not exceed £2 15s. This must very soon be reimbursed by the saving in repairs of the roof alone; and all the inconvenience, beside the injury done to the ceilings and goods, would be avoided. If coal tar were used, which, I should imagine would perfectly answer the purpose, supposing such a roof to require a hundred weight, this now sells for 18s., so that the cost would be only two guineas. C.

XIII.

Extract of a Letter from Prof. KRIES, *of Gotha, to Mr.* GEHLEN, *on Radiant Heat*[*].

I Imagine a short historical note on radiant heat will not be unpleasing to your readers.

Experiments on radiant heat in the 17th century.

The experiments of Pictet, made with two mirrors, in the focus of one of which he placed a burning body, and thus set fire to combustible substances in the focus of the other, justly excited the attention of natural philosophers. But it is surprising, that such experiments had been made more than a hundred years before, but were forgotten. Lambert, in his Pyrometry, says, on the authority of Zahn, that the experiment of collecting heat from a charcoal fire by a mirror of 18 inches diameter, and reflecting it to the distance of 20 or 24 feet to a smaller mirror of 9 inches, which so concentrated the rays, that tinder and matches were kindled by them, had long ago been made at Vienna. The work of Zahn referred to is no doubt his *Oculus artificialis*[*], which was published in 1685.

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[*] Ann. de Chim. vol. LXXI, p. 158. Translated from Gehlen's Journal by Tassaert.

Wooden mirrors of great power.

I have a small tract entitled: Account of the parabolic wooden Mirrors, and their surprising action, which were lately invented by Andrew Goertner, Machinist, and Model-Master to the King of Poland and Elector of Saxony, at Dresden, 1785.

Experiments with them.

In this work is the following description of an experiment. "I placed a live coal before the mirror, in its focus, and immediately the mirror diffused a strong heat to the distance of forty or fifty paces, which it would not do with the sun. Hence I conceive, that what is told of the celebrated Archimedes is true: though that he did not produce so powerful a heat by the help of the solar rays, but by a fire kindled for the purpose; for when I placed a small mirror, half an ell [near a foot] in diameter, opposite the greater, and sixty paces from it, and the coal was made to burn bright, immediately the little mirror lighted a candle; which many persons would not believe, till they had seen it. I tried also whether a lighted candle would produce the same effect as the coal; but it did not, for there was not the least heat from it."

Size of the mirrors.

The large mirror here alluded to was 2½ feet in diameter: the largest of the author's making was five.

Other experiments.

The following experiment appears to me still more surprising. The author says: "if I held the mirror ten or twelve paces from a hot iron stove, immediately it kindled a fire: and the same thing took place on holding it 20 or 24 paces from a fire in an open fire place."

Pictet's experiment with ice, which surprised him so much, is described in the same work.

Radiant cold.

A little after the experiments I have just mentioned, the author continues thus, "If, instead of fire, I placed cold water in the focus of the mirror, it diffused an agreeable coldness even in the height of summer: and if, instead of water, I used ice, very considerable cold was produced at the distance of ten or twenty paces.

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[*] More probably, I should think, his mundi Æconomia, or his Specula physicomathematicohistorica. C.

Structure of the mirrors.

Dolf, who has spoken of Gaertner's gilt mirrors, and after him Gehlen, and lastly Fischer, all say, that it is not known how Gaertner made his mirrors. The following is the account he gives in his own work.

"All these mirrors are of common wood; more of soft wood than of hard. For, though I have made mirrors of all sorts with metals, yet what I have said is to be understood of wooden mirrors only, which must be gilt within and without. If any amateur wish to make such mirrors himself, he has only to apply to the inventor, who will inform him and instruct him in every step necessary to be taken both for making and gilding them."

Gaertner therefore, though he does not tell us how he fabricated them, made no mystery of it. It is singular, that he directs the mirrors to be gilt on both sides. Perhaps this tended to preserve the mirror, and prevent its being warped by dryness: perhaps in this way he had a convex and a concave mirror in one: but he says nothing on the subject.

XIV.

Method of keeping Green Pease and French Beans.

Method of keeping green pease,

INTO a middling sized stewpan, filled with young green pease, put two or three tablespoonfuls of sugar, and place the pan over a brisk charcoal fire. As soon as the pease begin to feel the heat, stir them two or three times; and, when they yield water, pour them out on a dish, to drain off the water that comes from them. When drained, spread them out on paper in an airy room, where the sun does not come; and, that they may dry the more quickly, turn them frequently. It is necessary for their keeping, that they should not retain any moisture; if they do, they will soon grow mouldy.

and French beans.

French beans may be managed in the same way, and thus they will keep till the next season as well flavoured as when first gathered.

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* Sonnini's Bibliothèque Physico-économ. Aug. 1808, p. 105.

XV.

XV.

On the Art of Printing from Stone. Communicated by a Correspondent.

Art of printing from stone little known here.

IN your last number you inserted from the "Annales de Chimie" an account of the method of printing from stone. It is certainly an ingenious, and most probably a useful art; though I believe very little known in England. There are one or two circumstances respecting it, with which some of your readers may like to be acquainted, that Mr. de Serres has passed by. A close texture, as he observes, is advantageous, and indeed necessary to its giving a clean impression.

Ink for it.

Muriatic acid preferable to nitric.

I made the ink according to his direction, (which was considered so great a secret) but prefer to it coloured turpentine, copal, or lac varnish. Muriatic acid is cheaper than nitric acid, and has the advantage of not acting upon the resin or wax, which forms the base of the varnish used.

Choice of the stone.

After purchasing some pieces of marble, I was much vexed to find that both the muriatic and nitric acid left some of the veins untouched, and only partially dissolved others; this must be attended to in selecting the blocks. I find some pieces of the limestone from Clifton near Bristol take a tolerable polish, and dissolve readily.

Best method for small articles.

But the easiest and cheapest way for those who wish to have a card, ciphers, &c., is Chauvron's on stone or even on lead. A little piece may be executed in a quarter of an hour; and if wetting is not sufficient to prevent the ink from adhering to the block, it will bear sponging, and yet leave enough of the ink upon the figures.

The insertion of these hints in your valuable Journal will oblige your constant reader.

G. O.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Rare animals in Shetland.

AT a meeting of this Society, on the 19th of May last, the Rev. John Fleming, of Bressay, read an account of several rare animals found by him in Shetland; particularly pleuronectes punctatus, a specimen of which he exhibited to the Society; lucernaria quadricornis; echinus miliaris, &c.; and some undescribed species, particularly a flustra, which he proposed to call *flustra Ellisii*, in honour of Mr. Ellis, the illustrator of the corallines. At the same meeting, Dr. John Barclay read remarks on some parts of the structure of the large marine animal cast ashore in Stronsay last year.

Germination of grasses.

At a meeting on the 26th of May, Dr. John Yule read a summary of experiments and observations on the germination of the gramineæ, in which he stated some new facts respecting the economy of this useful class of plants, illustrated by a series of drawings and specimens of the germinated seeds of the cerealia or cultivated species, and of the buds of the stem, and panicle of viviparous grasses. And the Secretary read a communication from William Fitton, Esq., on the porcelain earth of Cornwall.

Copleyan Medal.

The Copleyan medal for last year was adjudged by the Royal Society to Mr. Edward Troughton, for the account of his method of dividing astronomical instruments, printed in the last volume of the Philos. Trans. See Journal, vol. XXV, p. 1 and 100.

Eye of the codfish.

Mr. Albers, of Bremen, having examined the eye of the codfish, gadus morrhua, L., finds, that the sclerotica is composed of two coats; the outermost of which is membranous, while the innermost is horny, and divisible into several layers. The innermost of these layers is coated with a substance resembling spermaceti, which forms little cysts, containing water in their intervals. This separates the sclerotica from the outermost silvery coat of the choroides, which by maceration in water becomes divisible into two layers. The intermediate coat of the three principal ones, that occupy the place of the choroides in the eyes of fishes, is composed in the cod of a retiform vascular texture, the trunks of which issue from the horseshoe shaped tubercle peculiar to fishes, and the use of which is so difficult to explain. Some

Tubercle in the eyes of fishes.

Some anatomists have taken this tubercle for a gland, others for a muscle; but it appears, from the minutest research, to be a convolution of the larger bloodvessels, and Mr. Albers considers it as a sort of reservoir for the blood, a kind of *rete mirabile*.

Air bladder of the tubfish.

The air bladder of the sapphirine gurnard, trigla hirundo, L., differs very remarkably in its internal structure, as well as in its shape, from that of any other known fish. It is near three inches long, two broad, and more than one deep. At one end it has the appearance of having received a deep gash, and at the other of two. The first is formed by a slight separation of about three quarters of an inch; the others by two perpendicular separations, nearly parallel, and more than two inches long.

Establishment for the education of the blind.

The loss of any one of the senses is certainly a serious calamity, but that of sight is on many accounts the most important. Every attempt to alleviate this loss therefore deserves encouragement, and may be expected to meet success proportionate to its merit. The establishment of Mr. Haüy at Paris, some remarks on which by Mr. Berard were given in our Journal, vol. III, p. 189, was formed near thirty years ago, and has been imitated we understand in other parts of Europe. The benefits, that have already resulted from the School for Indigent Blind in St. George's Fields, are no doubt known to many of our readers; but from a participation in these the children of the wealthy are excluded, nor indeed would they be adequate to their wants. We are happy therefore to learn, that an establishment is recently formed at Chelsea for teaching not only reading and writing, but the various sciences, to the opulent blind; so that the children of the rich, labouring under this misfortune, may have it alleviated as far as is practicable. The plan appears to be, on the whole, highly commendable: but, as it is yet in its infancy, the author, Mr. Bonner, would be happy to receive any suggestions, that could tend to its improvement.

To CORRESPONDENTS.

Mr. Shute's paper was too late for insertion this month.

Meteorological

METEOROLOGICAL JOURNAL,

For JULY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JUNE Day of	THERMOMETER.				BAROMETER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night		Day.	Night.
27	58°	58°	63°	50°	30·01	Fair	Fair
28	59	60·5	66·5	55	30·30	Ditto	Cloudy *
29	64·5	60	68	51	29·98	Rain	Ditto
30	60	64	70·5	56·5	30·13	Fair	Ditto
JULY							
1	54·5	66	74·5	59	30·11	Fair †	Rain
2	64	63	72·5	56	29·86	Ditto	Fair
3	64	56	65	51·5	29·67	Ditto	Ditto
4	58	56	61	51	29·39	Rain	Ditto
5	59	62	65·5	58	29·77	Ditto	Ditto
6	63	63	69·5	54	29·91	Fair	Ditto
7	63	67	74·5	61	30·03	Ditto	Ditto
8	67	61·5	68	55	29·80	Rain	Ditto
9	62	63·5	70	58	29·95	Fair	Ditto
10	64	63·5	68	59·5	29·76	Ditto	Ditto
11	65	65	73·5	58	29·64	Ditto	Ditto
12	64	64	68	58	29·56	Ditto	Cloudy ‡
13	64·5	64	70	55	29·61	Rain	Fair
14	63	55	65	48·5	29·71	Ditto §	Ditto
15	55·5	56	66	48	29·95	Ditto	Ditto
16	59	57	62	50·5	30·03	Ditto	Rain
17	55	58	62·5	51	29·82	Cloudy	Cloudy
18	56	58	64	51·5	29·58	Fair	Fair
19	57·5	60·5	65	51·5	29·84	Ditto	Ditto
20	59	58·5	65	51	29·79	Ditto	Ditto
21	58	51	60·5	48·5	29·92	Ditto	Ditto
22	56	59	63	50	30·10	Ditto	Ditto
23	59	60·5	70	54	30·17	Ditto	Ditto
24	61	61·5	73	53·5	30·17	Ditto	Ditto
25	65	64·5	71·5	59·5	29·88	Rain	Cloudy
26	66	56	66·5	53	29·80	Ditto	Ditto

* Thunder, lightning, and heavy rain in the night.

† Storm commencing about 3 P M Boisterous at 8, with thunder and lightning, continuing most of the night, with rain.

‡ Rain at half past 11 o'clock.

§ At 2 P. M storm of hail, rain, thunder, and lightning, the thermometer retiring to 55°. Some hail stones near half an inch diameter.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXVI.

ARTICLE I.

The Bakerian Lecture for 1809. *On some new Electrochemical Researches, on various Objects, particularly the metallic Bodies from the Alkalis, and Earths, and on some Combinations of Hidrogen, By* HUMPHRY DAVY, *Esq. Sec. R. S. F. R. S. E. M. I. R. A.*[*]

I. *Introduction.*

Application of electricity to chemistry pursued.

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries, on the application of electricity to chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication, I shall, as formerly, state the results. I hope they will be found to lead to some views and applications, not unconnected with the object of the Bakerian lecture: and though many of them are far from having attained that precision and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

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[*] Philos. Trans. for 1810, p. 16.

Explanation of the Figures.

Explanation of the plates.

Pl. IX, Fig. 1. The apparatus for electrizing potassium in gasses. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water out of the contact of air. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the VOLTAIC electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a*, *d*, *c*, the level of the different fluids.

Pl. X, Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A VOLTAIC apparatus, being one of the 200 which compose the new VOLTAIC battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of £1000 has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected, from so great an accumulation of electrical power.

II. *Some new Experiments on the Metals from the fixed Alkalis.*

Metals of the fixed alkalis.

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Different hypothetical explanations of the facts.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

Messrs. Gay-Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hidrogen; a similar opinion seems to be entertained by Mr. Ritter. Mr. Curaudau[*] affects to consider them as combinations of charcoal, or of charcoal and hidrogen, with the alkalis; and an inquirer [†] in our own country regards them as composed of *oxigen* and *hidrogen*.

I shall examine such of these notions only as have been connected with experiments, and I shall not occupy the time of the Society with any criticisms on matters of mere speculation.

Gay-Lussac and Thenard's founded on the action of potassium on ammonia.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which Messrs. Gay-Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mém. d'Arcueil, a copy of which Mr. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail it seems, that they still retain their opinion; but upon precisely the same grounds as those, to which I have before referred. That no step of the discussion may be lost to the Society, I shall venture to state fully their method of operation, and of reasoning.

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[*] Journal de Physique, June, 1808; or Journal, vol. xxiv, p. 40.

[†] Nicholson's Journal, August, 1809, p. 258

Their method of operation,

They say, that they heated potassium* in ammonia, and they found, that a considerable quantity of ammonia was absorbed, and hidrogen produced; and that the potassium became converted into an olive coloured fusible substance. By heating this substance strongly, they obtained three fifths of the ammonia again, two fifths as ammonia, one fifth as hidrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two fifths, and found in the vessel, in which the operation was carried on, nothing but potash.—Again, it is stated, that, by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hidrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm, that they can procure from a given quantity of ammonia more than its volume of hidrogen.

and reasoning on it.

Whence, they ask, can the hidrogen proceed?—shall it be admitted, that it is from the ammonia? but this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water, which may be supposed to be in the ammonia, or from the metal itself. But the experiments of Mr. Berthollet, jun., prove, that ammonia does not contain any sensible quantity of water. Therefore, say they, the hidrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hidrogen, and that alkali.

Defect in their statement.

It is obvious, that, even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm, that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxigen which this would give to the potassium, and of the hidrogen which it might furnish, to reproduce the ammonia, they take no notice.

Mr. Davy's results very different,

I have shown, by numerous experiments, many of which have been repeated before members of this Society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

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* Mém. d'Arcueil, Tom. II, p. 309.

from cautious proceeding.

Little ammonia regenerated,

In proportion as more precautions are taken to prevent moisture from being communicated to it, so, in proportion, is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hidrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

and an excess of nitrogen produced

Potassium always revived.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show, that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed page 458 of the last volume of the Transactions*, and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

Potassium heated in ammonia

I employed a tube of platina bored from a single piece, which, having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort: the potassium was employed in quantities of from 3 to 4 grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

rapidly,

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation; and in these cases, there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment. But when the heat was very slowly raised, the loss was much less considerable, and in several cases, I obtained more than four fifths of the potassium, which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

and very slowly.

Experiment detailed.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30·2 in.; thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7·5 were absorbed, and 3·2 of hidrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hidrogen. They contained together $\frac{5}{10}$ of a cubical inch. The heat was very slowly applied, by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxigen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed was such as to generate, by its action upon water, 3 cubical inches and $\frac{1}{10}$ of hidrogen gas.

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* Journal, vol. xxv, p. 143.

Results.

Now if this experiment be calculated upon, it will be found that 7·5 — ·2 = to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hidrogen. But the 3·2 cubical inches of hidrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process = 9; and the nitrogen in the 8·8 cubical inches of gas, (or the 9 — ·2 of ammonia,) will be about 3; and if we estimate ·34 of hidrogen, and ·16 of nitrogen, in the 5 remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the $\frac{8}{10}$ of hidrogen preexisting in the tube and adaptors, the loss of hidrogen will be found proportionably rather greater than that of nitrogen.

Another experiment.

In another experiment, in which 3 grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors the same quantity of residual air, as in the process last described.

The

Results.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxigen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2 in., thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Deductions from them.

Now 6·78—·4 of ammonia = 6·38, and this quantity of gas, decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hidrogen. But there are produced in this experiment, of hidrogen, 2·48 in the first operation, and 4·28 in the second; and, considering the nitrogen in the permanent gas as 3·32,0·8 must be subtracted from the common air; which would give 2·52 for the nitrogen generated: and to these must be added the quantity of hidrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hidrogen.

Black matter separated.

In all experiments of this kind a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This examined.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina. I exposed some of it to heat in a retort, containing oxigen gas; there was a diminution of the gas, and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

A compound of platina (and hidrogen?)

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found, that in the process in which from 3 to 4 grains of potassium were made to act upon ammonia in a vessel of platina, and afterward distilled in contact with platina, there were always from 4 to 6 grains of this powder formed; but I have advanced no farther in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxigen.

In iron tubes, nitrogen lost, and hidrogen produced;

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hidrogen. When copper tubes are employed, the hidrogen bears a smaller proportion to the nitrogen; and more potassium is revived.

in copper, less hidrogen;

in platina, hidrogen lost.

In those experiments, in which platina has been used, there is little or no loss of potassium or nitrogen: but a loss, greater or smaller, of hidrogen.

On what do these results depend?

It will be asked, on what do these circumstances depend? Do the affinity of certain metals for potassium prevent it from gaining oxigen from ammonia, and do platina and copper combine with a small quantity of hidrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia it is not a *metallic* body that is decomposed, but the volatile alkali; and that the *hidrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxigen and hidrogen.

Action of sodium on ammonia.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3·3 grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hidrogen; and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{10}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced equalled twelve cubical inches;

and, by detonation with oxigen, proved to consist of nearly two of hidrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

The volatile alkali decomposed.

Whoever will consider with attention the mere visible phenomena of the action of sodium on ammonia cannot, I conceive, fail to be convinced, that it is the volatile alkali, and not the metal, which is decomposed in this process.

Phenomena of the action of sodium on ammonia.

As sodium does not act so violently upon oxigen, as potassium, and as soda does not absorb water from the atmosphere with nearly so much rapidity as potash, sodium can be introduced into ammonia much freer from moisture than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green; it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form evidently by combining with one part of the elementary matter of ammonia, while another part is suffered to escape in the form of hidrogen.

Mr. Curaudau's hypothesis

It will not be necessary for me to enter into a very minute experimental examination of the opinion of Mr. Curaudau, that the metals of the alkalis are composed of the *alkalis* merely united to *charcoal*. The investigation upon which he has founded his conclusions is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which Messrs. Gay-Lussac and Thenard have been misled by the moisture, which interfered with their results.

refuted.

Mr. Curaudau states, that, when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in 8 cubical inches of oxigen: nearly two cubical inches of oxigen were absorbed, and soda in a state of extreme dryness, so that it could not be liquified by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena, which I have described in the Bakerian lecture for 1807 *. Nearly 6 cubical inches of hidrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalis, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination, and it appears from Mr. Curaudau's method of decomposing the alkalis, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

Mr. Ritter's hypothesis

refuted.

Mr. Ritter's argument in favour of potassium and sodium being compounds of hidrogen is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxigen than potassium, and, on the hypothesis of hidrogenation, must contain much more hidrogen; yet, though soda is said to be lighter than potash in the proportion of 13 to 17 nearly †, sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxigen than sodium, and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

His observations on the action of tellurium upon water.

Mr. Ritter has stated, that, of all the metallic substances he tried for producing potassium by negative voltaic electricity, tellurium was the only one, by which he could not procure it. And he states the very curious fact, *that*, when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxigen is given off at the positive surface; no hidrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives, that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hidrogen, than that alkali *.

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* Journal, vol. xx, p. 307.

† Hassenfratz, Annal. de Chem, tom. xxviii, p. 11.

This does not prove potassium to be a compound.

These circumstances of the action of tellurium upon water are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find, that, instead of proving that potassium is a compound of potash and hidrogen, they confirm theidea of its being as yet, like other metals, undecomposed.

Experiments on it, the tellurium being positively and negatively electrified.

When tellurium is made the positive surface in water, oxigen is given off. When it is made the negative surface, the voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hidrogen in water; which, in being diffused, is acted upon by the oxigen of the common air, dissolved in the water, and gradually loses a part of its hidrogen, and becomes a solid hydruret of tellurium. The compound of hidrogen and tellurium produced at the negative pole, when uncombined, is gaseous at common temparatures; and when muriatic acid, or sulphuric acid, is present in the water, it is not dissolved, but is given off, and may be collected and examined.

A compound of tellurium and hidrogen produced.

Experiment with tellurium negatively electrified.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found at the beginning of this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which, when touched by water, did not inflame or effervesce, but rendered the water of a beautiful purple colour, and when thrown into

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* Journal, vol. xxiv, p. 318.

water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hidrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium; and that the *telluretted* hidrogen, (if the name may be used,) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water: and this I found to be the case; for, on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hidrogen; metallic tellurium was formed where it came into contact with the air, and muriate of potash was found dissolved in the mixture.

A compound of tellurium, hidrogen, oxigen, and potassium, produced.

Action of potassium on tellurium.

It seemed evident from this fact, that, in the action of tellurium negatively electrified upon potash, potassium was produced, as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was farther confirmed by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hidrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hidrogen was evolved by the action of the alloy upon water; but the compound of telluretted hidrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

Oxides of potassium and tellurium revived together by charcoal.

The very intense affinity of potassium and tellurium for each other induced me to conceive, that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time by heated charcoal; and I soon proved, that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

Accidental alloy of tellurium with potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hatchett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found, that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

Aeriform compound of tellurium and hidrogen.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aeriform* compound of tellurium and hidrogen; I shall mention merely some of its most remarkable qualities and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hidrogen is more analogous to sulphuretted hidrogen, than to any other body. The smell of the two substances is almost precisely the same*. Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened

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Supposition, that tellurium might contain sulphur.

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children Esq., of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton cooperated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive, that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of voltaic electricity, or of potassium; and some researches made upon the habitudes of different metallic sulphurets, at the voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hidrogen in the voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxigenation, could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and farther researches, made in the laboratory of the Royal Institution, proved, that the substance in question was a new and singular combination.

litmus; but, after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalis. It precipitates most metallic solutions. It is instantly decomposed by oximuriatic acid, depositing a film, at first metallic; but which is soon converted into muriate of tellurium*.

Arsenic

As arsenic has an affinity for hidrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

made the negative surface in water,

Arsenic made the negative surface in water, by means of a part of the new battery containing 600 double plates, became dark coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

in a solution of potash,

Arsenic negatively electrified in a solution of potash likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid.

and in contact, with solid potash.

When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hidrogen by the action of water with inflammation, and deposited a brown powder.

Potassium and

When potassium and arsenic † were heated together in hidrogen gas, they combined with such violence, as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the voltaic battery.

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* From the results of one experiment, which I tried, it seems that tellurium, merely by being heated strongly in dry hidrogen, enters into combination with it. An accident prevented me from ascertaining, whether the compound so formed is exactly the same as that described in the text.

Volatile pyrophorus from

† In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash

arsenic heated in hidrogen gas.

As tellurium and arsenic both combine with hidrogen, it appeared to me probable, that, by the action of alloys of potassium with tellurium and arsenic upon ammonia, some new phenomena would be obtained, and probably still farther proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

Action of alloy of tellurium and potassium on ammonia.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four sixths nitrogen, instead of being pure hidrogen, as in the case of the action of potassium alone.

Action of alloy of arsenic and potassium on ammonia.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas, which was principally nitrogen, so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hidrogen, which are contradictory assumptions.

Hidrogenation of potash not proved by analysis or synthesis.

None of the chemists, who have speculated upon the *imaginary hidrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon

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potash and white oxide of arsenic, Fourcroy, Connais. Chem. Tom. viii, p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that, though potash is decomposed in this operation, yet the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hidrogen. The gasses not absorbable by water, given off in this operation, are peculiar. Their smell is intensely fœtid. They are inflammable, and seem to contain charcoal, arsenic, and hidrogen: whether they are mixtures of various gasses, or a single compound, I am not at present able to decide.

oxide of arsenic and acetate of potash.

distant analogies, or upon experiments in which agents, which they did not suspect, were concerned. No person, I believe, has attempted to show, that, when potassium or sodium is burnt in oxigen gas, water is formed; or that water is generated, when potassium decomposes any of the acids*: and no one has been able to form potassium, by combining hidrogen with potash. I stated in the Bakerian lecture for 1807, that, when potassium and sodium were burnt in oxigen gas, *the pure* alkalis were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxigen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed, yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find, that this is the case; for the results differ very little in some experiments, which I have made, upon considerable portions of potassium and sodium, procured by chemical decomposition.

Oxigen consumed proportional to the quantity of metal.

When potassium is burnt in trays of platina, in oxigen gas that has been dried by ignited potash, the absorption of oxigen is about $\frac{11}{20}$ of a cubical inch for every grain of the

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Decomposition of boracic acid.

* When, in October 1807, I obtained a dark coloured combustible substance from boracic acid, at the negative pole in the voltaic circuit, I concluded, that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808 I made farther experiments on this substance, and ascertained, that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance, as I had procured by electricity. Messrs Gay-Lussac and Thenard have likewise operated upon boracic acid by potassium, and they conclude, that they have decomposed it; but this does not follow from their theory, unless they prove, that water is given off in the operation, or combined with the borate of potash; the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made hydruret of boracic acid.

metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain*. The alkalis so formed are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalis, give indications of the presence of moisture.

Potash and soda contain much water.

Potash loses its water when combining with muriatic acid.

Mr. d'Arcet has shown by some very well conducted inquiries, that potash and soda †, in their common state, contain a considerable proportion of water; and Mr. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in 100 parts 66·66 potash and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

Potassium converted into potash upon the surface of muriatic acid,

and in muriatic acid gas. Exp. 1.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hidrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances I obtained from 10 parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash in muriatic acid gas. I shall give the results of two experiments made in this manner: 5 grains of potassium, inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, burnt with a beautiful red light*, and the whole mass appeared in igneous fusion; a little muriate of potash, in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about 5 of hidrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

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* The quantities of gas given out by the operation of water are in a similar ratio. See page 43 of the last Bakerian lecture [Journal vol. xxiii, p. 245], and page 330 of this paper.

† Annales de Chimie. Nov. 1808, page 175; or Journal for September.

Exp. 2.

The second experiment was conducted with still more attention to minuteness. 8 grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was 6½ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium, where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal ⅛ of a grain.

Deductions.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxigen, to form 9·4 grains of potash, and 6·6—1·4 = 5·2, the quantity of muriatic acid combined with the potash; which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash. But 35·6 of muriatic acid, according to Mr. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash; and 71·1—64·4 = 6·7; so that the potash taken as a standard by Mr. Berthollet contains at least 9 per cent more water, than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali[*].

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* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

After these illustrations, I trust the former opinions, which I ventured to bring forward, concerning the metals of the fixed alkalis, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

Potash and soda pure metallic oxides.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hidrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalis, and oxides. This part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

(To be continued.)

II.

Of the Spinellane: and some other fossil Substances, by Mr. NOSE[†].

MR. NOSE has described this substance in his mineralogy of the mountains of the Rhine. He found it on the banks of the lake of Laach, near Andernach. It is in a rock composed of various substances, as oxide of iron, quartz, hornblende, mica, and some other substances, to which he has given peculiar names, such as,

Spinellane where found.

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[*] Consequently Mr, Berthollet's fused potash must contain nearly 23 per cent of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent of water, taking the potash formed by the combustion of potassium as the dry standard.

Water in potash.

[†] Journal de Physique, vol. lxix, p. 160.

Sanidin. 1, A variety of tabular feldspar, which he calls *sanidin*;

Desmin. 2, Another substance, crystallized in small silky tufts, which he calls *desmin*.

Characters of the spinellane. The spinellane has a brownish colour.

Its form appears to be that of a hexaedral prism, terminated by triedral pyramids with rhomboidal faces.

Mr. Nose however imagines, that he has observed a great many points of resemblance between it and the balass ruby, or spinelle, whence he has been led to give it the name of spinellane.

It does not rank very high in point of hardness, but is sufficiently hard to scratch glass.

III.

Researches on Acetic Acid, and some Acetates: By RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A. &c.*

(Concluded from p. 237.)

Saturating power of the solution of potash ascertained. TO ascertain the saturating power of this solution of potash however, I neutralized with it muriatic acid diluted so as to be of the specific gravity of 1·0707. Of this 10·000 parts required 27·448 of solution of potash. I then precipitated an equal quantity of the same acid by nitrate of silver, and I obtained 5·050 of muriate of silver, which gives us a fixed standard for the solution of potash, and renders the experiments capable of being compared together, and with others. The specific gravity of the solution of potash was 1·0786.

Separation of the spirituous liquor by carbonate of potash. Carbonate of potash thrown into a fluid containing a spirituous liquor is divided into two portions: but if the fluid contain acetic acid also, or, I believe, any other acid, that forms with the potash a salt soluble in spirit, the separation does not take place; particularly if the acid contained in it be much more in quantity than the spirituous liquor. For this reason I have been forced to distil the fluid products of acetates of silver, copper, and nickel, from potash, till all their acid was extracted, and examine them afterward for the spirituous liquor.

I introduced a hundred parts, by measure, of the liquid to be assayed into a tube, which was 5 dec. [19·6 inch.] long, and 5 or 6 mil. [0·2 of an inch] in diameter, and closed each end by a small cork. I put in carbonate of potash sufficient to render the separation complete, and in order to bring the spirituous liquor to the part of the tube, where I had measured the whole of the liquid before I put in the carbonate of potash, I let out a portion of the liquid below, by drawing out the cork from the bottom, so as to compensate the increase of bulk occasioned by the addition of carbonate of potash. Thus I avoided the errours, that any inequality in the bore of the tube would have occasioned.

The pyroacetic spirit thus separated is not at its greatest degree of dryness: but it is in a sensibly uniform state, and may be subsequently rectified by other processes.

In a similar way I proceeded with all the liquid mentioned in this paper.

Aeriform products.

As to the aeriform products, the separation of the carbonic acid was effected in the solution of barytes contained in the first phial. I ascertained its quantity by examining the carbonate of barytes formed during the process. The hidrogen gas I collected in the pneumatic apparatus.

Calculations.

I will not assert, that there is exactly the same precision with regard to all the acetates; but the variations appeared to me too small, to be noticed in researches of this kind. If an acetate of the same metal be distilled several times, we shall find slight variations from one experiment to another, which require numerous repetitions, that we may take the mean of as many operations as possible. We should also take every care, to preserve the same degree of temperature throughout every operation, as well as in all that are undertaken with a view of comparing the different acetates with each other.

These variations take place most generally in the acetates that yield the greatest quantity of pyroacetic spirit; and it was by taking the mean terms of several operations, and particularly attending to the greatest and smallest quantities of each substance produced in distilling all these salts, that I determined the proportions and order in the following general table.

Tabulated results.

		Acetate of silver.	Acetate of nickel.	Acetate of copper.	Acetate of lead.	Acetate of iron.	Acetate of zinc.	Acetate of manganese
	Loss in the fire.	0·36	0·61	0·64	0·37	0·49		0·555
Residuum in the retort.	State of the base.	Metallic.	Metallic.	Metallic.	Metallic.	Black oxide.	White oxide.	Brown oxide.
	Residual carbon.	0·05	0·14	0·055	0·04	0·02	0·05	0·035
Liquid products	Specific gravity.	1·0656	1·0398	1·0556	0·9407	1·011	0·8452	0·8264
	Proportion of acidity.	107·309	44·731	84·868	3·045	27·236	2·258	1·285
	Spirituous liquor.	0	2	0·17	0·555	0·24	0·695	0·94
Aeriform products.	Carbonic acid.	8	35	10	20	18	16	20
	Carburetted hidrogen.	12	60	34	8	34	28	32
	Total of gas.	20	95	44	28	52	44	52

Anomaly in the acetate of silver.

In this table it may be remarked, that the specific gravity of the product of the distillation of acetate of silver is but 1·0656, while its degree of acidity greatly exceeds that of the rest: yet it does not contain any sensible portion of pyroacetic spirit. At first I suspected, that this product might contain some other vegetable acid, beside the acetic: and I had not to choose among a great number, for few would have resisted the heat this product had undergone without being decomposed or volatilized. I saturated a portion with potash, and sought in vain for some other acid. I chiefly expected to find in it pyrotartaric acid, but it did not form the least precipitate with acetate of lead.

This fact may be explained by the tendency the concentrated acetic acid has to become solid, and the expansion it would undergo a little before the instant of congelation, analogous to what sir Charles Blagden observed in water. I exposed to the same temperature the products of the distilled acetates of silver, nickel, and copper, and that from silver crystallized first. It was likewise the last liquefied on raising the temperature anew; which tends slightly to support the explanation I have given.

Specific gravity of different compounds of the acid from it with water.

With 15 gram. [232 grs.] of the liquid product of the distillation of the acetate of silver I mingled water by a gramme at a time. The specific gravity went on increasing, till three grammes of water had been added, when it was 1·0733; and the degree of its acidity, according to the component parts, 76·8959. With five grammes of water the spec. grav. was 1·0693. From five grammes to ten there appeared to me some slight variation; but beyond that proportion, when it was 1·0597, it decreased uniformly. I had no opportunity of examining this series of mixtures but once, but it appears to favour my opinion. It must be confessed however, that the effect is very great to be produced by so slight a cause.

Mixtures of acetic acid and water should be examined.

This part of my researches however I have not examined sufficiently to decide. It is to be wished, that the exact ratios between the specific gravity and acidity of a liquor containing acetic acid, without any other substance, should be ascertained*.

Crystallization of acetic acid not owing to spirit.

What I have just said of the congelation of the liquid product of the distillation of acetate of silver proves, that the crystallization of acetic acid is no way owing to the presence of the spirituous liquor. In fact how can we imagine, that a fluid congealable only at a very great degree of cold can increase the crystallizableness of another fluid, which crystallizes much more readily than itself? No cause for the crystallization of tartaric, oxalic, or any other acid has ever been sought for, but its peculiar nature.

Purest and strongest from acetate of silver.

The purest and most concentrated acetic acid I ever saw was that I obtained from the acetate of silver by distillation.

Acetate of nickel.

There seems to be some anomaly in the products of the distillation of acetate of nickel. The quantity of carbon in the residuum in the retort is very great, and there is likewise a great deal carried off by the gas. The difficulty of obtaining a sufficient quantity of this metal prevented me from carrying my examination farther.

Little spirit from the acetates of silver, nickel, and copper,

I cannot answer to a few hundredths for the quantity of spirituous liquor contained in the products of the distillation of the acetates of silver, nickel, and copper, on account of the small quantity they contain, and the larger proportion of their acetic acid. The acetate of silver appears to me to contain none. I am not so certain with regard to that of nickel. After having deprived the liquid product of the acetate of copper of a large portion of its acid by distilling it with carbonate of potash, I separated about 0 17 of spirituous liquor by the method used for the other liquid products.

and iron.

The acetate of iron is one of those most easily decomposed by heat. Accordingly it yields less of the spirituous liquor, and more acid, in the product of its distillation, than we have reason to expect from its other properties.

More in proportion as the metal less easily reducible.

Without arrogating to these results more confidence, than the nature of the research allows them to claim, we see, that from the four metals reduced to the metallic state during the operation the quantity of pyroacetic spirit is uniformly greater in proportion as the metal is less easily reducible. Zinc, which does not alter its state, gives still more: and in this case the heat is necessarily greater, before the acid quits its base. Lastly manganese, which is not in the same state before and after the process, yields more of the spirituous liquor than zinc, because the base is capable of absorbing about 0·46 of oxigen, and with this the acid furnishes it.

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* See the paper by Mr. Mollerat, Ann. de Chim., vol. lxviii, p. 88; or Journal, vol. xxv, p. 155.

Has less oxigen than acetic acid.

Some preliminary experiments, by which I found that acetic acid contains a larger proportion of oxigen than the pyroacetic spirit, prove, that this acid furnishes it for the superoxidation of the oxide of manganese; and that by this mean an effect is produced, which in other circumstances takes place without it.

Liquid from the alkaline acetates more spirituous and less acid than from the metallic.

From acetate of barytes pure spirit alone.

This will be farther demonstrated by the distillation of earthy and alkaline acetates. I distilled the acetates of potash and soda, and obtained a more spirituous and less acid liquor, than any of those furnished by the metallic acetates. I obtained a similar result by the distillation of acetate of lime; but very pure and dry acetate of barytes, prepared with acetic acid made by Mr. Mollerat[*], yielded me a liquid of the spec. grav. of 0·8458, which did not redden blue vegetable colours, and which, being treated with carbonate of potash to separate the pyroacetic spirit, did not allow the separation of a single drop. So far from this, having mixed a hundred measures of it with a hundred measures of water, and having afterward added carbonate of potash in the usual way, more than a hundred measures of pyroacetic spirit separated. The whole of the liquid therefore, arising from the distillation of acetate of barytes, was pyroacetic spirit more free from water than that from which carbonate of potash has taken all the water it can. Its specific gravity was greater than that of pure pyroacetic spirit, because it contained a yellow empyreumatic oil.

In the inverse ratio of the facility of decomposition.

These results prove, that, *cæteris paribus*, the production of pyroacetic spirit follows pretty exactly the inverse ratio of the facility of decomposition by a high temperature: and that, if the acetate of manganese be an exception to this law, it is because its base facilitates the decomposition of the acid by absorbing oxigen.

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[*] Consequently obtained from the distillation of wood. C.

No doubt there are other circumstances, that influence the results of these distillations; but the determination of such complicated causes is a problem not to be solved in the present state of chemistry.

Uniform in its properties.

Pyroacetic spirit is precisely the same substance, whatever be the salt that has served for its formation.

Products of acetates examined for prussic acid,

Prussic acid and ammonia have been classed among the products of the distillation of acetates. I have digested the liquid product of the acetates of lead and of potash on the red oxide of mercury, and on the black oxide of iron with potash. I have treated it with sulphate of iron, and by all the means I could think of; but I could not find any prussic acid, any more than in the dry residuum in the retort. Though it is extremely difficult, to detect very small quantities of prussic acid in any substance whatever, I do not think any exists in the liquid products of distilled acetates.

and for ammonia.

Visible vapours from muriatic acid no certain test of it.

As to the ammonia I added lime and potash to the liquid products of several acetates, and I afterward held near them a tube wetted with muriatic acid, when very visible vapours were produced. I could not distinguish the ammonia by the smell. I held the same tube moistened with muriatic acid near the surface of a little common alcohol, and observed the same vapours. I precipitated by a solution of potash a solution of acetate of copper, and added a little of the liquid product of distilled acetate of potash. Dividing it into two portions, into one I put a single drop of ammonia, and none into the other. I filtered the two separately, and then passed a stream of sulphuretted hidrogen gas through each. In that containing the drop of ammonia, brown flocks were formed, like hidrosulphuret of copper; but in the other I could distinguish nothing. From these experiments I am persuaded, that no ammonia is formed in the distillation of acetates; and that the mistake has arisen from the vapours, which muriatic acid forms with the spirituous and volatile part of their respective products.

Found perhaps when the acid was impure.

It is of little importance to know whether prussic acid or ammonia be contained in the products of acetates formed by an impure acetic acid like vinegar; for the extractive or mucilaginous matter might furnish either, without the acetic acid contributing to it. But as this acid is frequently the last result of the destructive distillation of vegetable substances, it is essential to know, whether it contain nitrogen.

Brown fetid oil from acetates.

In all the products of the distillation of acetates, but particularly those that yield most pyroacetic spirit, we find a brown, fetid oil; but I have taken no notice of this in the table.

Acetate of lead distilled with charcoal,

I endeavoured to modify the products of the distillation of the acetate of lead, by mixing with it other substances, either oxidating, or disoxidating. The acetate of lead mingled with a fourth of charcoal yielded 42.5 of pyroacetic spirit by distillation: and the same salt mingled with a fourth of black oxide of manganese yielded but 34. Their specific gravities were 0·9606 and 0·9633; and their ratio of acidity 2·445 and 2·052.

and with oxide of manganese.

The acetate of lead described by Thenard yielded 59 of pyroacetic spirit; 3·973 of acidity; and the spec. grav. of the whole liquid product was 0·9302.

Heat alone does not convert pyroacetic acid into pyroacetic spirit.

It is certain, that heat alone is not sufficient, to convert acetic acid into pyroacetic spirit; but that the concurrence of other circumstances is necessary, which we are not yet able to ascertain. Into a porcelain tube, at a strong red heat, I passed acetic acid of the spec. grav. 1·0635, and the acidity of which was 60·624. The porcelain tube was luted to a tubulated matrass, and in the tubulure were placed two tubes. One of these, being open at both ends, allowed me to drain out the liquor, that condensed in the matrass, by means of a little pipe. The other, which was curved, reached to the bottom of a phial containing a solution of barytes, and communicating with a pneumatochemical trough. I continued the process eight hours, constantly returning into the retort the liquid that condensed in the matrass; so that ultimately each particle had passed five or six times through a tube heated red hot for the space of six inches. Carbonic acid and carburetted hidrogen gas were continually evolved. The liquid that passed into the matrass became more and more brown, and a small quantity of coally matter was left in the retort. The weight of the acid was then 1·0443, and the proportion of its acidity was only 33·65. No pyroacetic spirit was formed. Acetic acid therefore is capable of supporting a great heat, without being totally decomposed, and is at the same time highly volatile. For this reason it is almost always a result of the destructive distillation of vegetable and animal substances.

Heat diminishes its acidity more than its gravity.

It is to be observed that the acidity of the acetic acid, that has passed through a red hot tube, diminishes much more than its specific gravity. This is analogous to what happens in the result of distilled acetate of nickel. The cause I know not. I could not find any oil or other substance in these liquids; if they contain any other acid than the acetic, it must differ from this in a very few properties, for all those I examined agree perfectly with those of acetic acid.

Vapour of acetic acid passed through charcoal.

If carbon be introduced into the red hot porcelain tube before the vapour of acetic acid is passed through it, nothing is collected, even after a single distillation, but water, carbonic acid, and carburetted hidrogen gas.

The spirit obtained from acetic acid alone;

The distillation of some other salts formed by vegetable acids leads me to believe, that the acetic is the only acid, the salts of which furnish pyroacetic spirit by distillation.

not from tartaric,

I have distilled tartrate of copper and lead, and acid tartrate of potash. I have treated the products in the same manner as I did those of the acetates, but in no instance did I perceive any traces of the pyroacetic spirit.

oxalic,

The acid oxalate of potash also afforded me none. On comparing all the products of the distillation of the tartrate and oxalate of potash, I believe, that the tartaric acid differs from the oxalic chiefly by containing a larger proportion of carbon.

or citric.

I could not obtain any pyroacetic spirit from the distillation of citrate of potash.

Compared with other spirits.

As the pyroacetic spirit appears to be a constant and uniform substance, whatever may be the acetate from which it is derived, I have compared it with other spirituous liquors, in which the acetic acid might have had some influence.

Formation of acetic ether.

It is pretty generally imagined, that what is contained in vinegar is the result of the vinous fermentation, which during the distillation becomes ether by the action of the acetic acid. Mr. Gehlen very lately denied the direct formation of an acetic ether: but Mr. Thenard has since formed it, as other chemists had done before*. In March 1803 I obtained the following results, and I have since verified them.

Experiment.

I mixed together ten parts of alcohol, of the specific gravity of 0·8483, and ten of acetic acid exempt from all spirituous liquor, the spec. grav. of which was 1·0705, and 10 parts of which were neutralized by a quantity of base, which I shall represent by 49·587. The specific gravity of the mixture was 0·9450, though by calculation it should have been 0·9594. There was a very slight evolution of caloric, but I observed no other action, even at the expiration of eight and forty hours. I afterward distilled this mixture to dryness. The product had lost nothing perceptible of its weight, but its spec. grav. had become 0·9372. I distilled this liquid to dryness eight times over in close vessels; but its specific gravity did not alter after the first distillation. I neutralized 10 parts of this ether, which from its composition was equal to 5 parts of the acetic acid employed; and it took but 14·274 of base, instead of 24·793, the half of 49·587. Thus on distilling together 10 parts each of such alcohol and acetic acid as are described above, $\frac{10519}{247935}$, or nearly $\frac{5}{12}$, of the acid were expended in the etherification.

The ether freed from acid.

To procure this ether free from acid, I saturated it with dry carbonate of potash; and added enough of this salt to take up all the water. Thus I obtained 7·40 parts of ether of the specific gravity of 0·8621. By adding a little of this to highly concentrated acetic acid, we have in a small

Concentrated vinegar.

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* For its spontaneous formation see p. 157.

bulk

I found acetate of potash, with some potash not saturated.

Pyroacetic spirit treated in the same way.

Into an equal quantity of pyroacetic spirit of the specific gravity of 0.8086 I put a great deal more caustic potash, which dissolved in it slowly, and the liquor became of a very bright yellow. I left it thus at least a fortnight. The whole of the potash was dissolved, the liquor was grown much deeper coloured, and its smell was more fragrant, though not fundamentally changed. On distilling this liquor, it came over very clear and colourless, with the same smell, and all its other characteristics.

Farther comparative experiments.

On preparing the potash in Mr. Berthollet's manner, the alcoholic solution became browner in proportion to its being concentrated; and at length carbon was formed, which floated on a very clear and colourless fluid, but at length disappeared. The solution of potash in pyroacetic spirit never lost its colour; but, on distilling it to dryness, a brown mass remained in the retort. This brown mass I dissolved in water, and again reduced to dryness. It was brown and shining. Exposed to the air for eight and forty hours in a platina capsule it did not perceptibly attract moisture; its taste was a little saponaceous and acrid; and acids threw down from it a yellow flocculent precipitate.

I dissolved a fresh quantity of potash, in the liquor that I had already treated with this alkali, and distilled it afresh in the manner described. The phenomena were precisely the same. Thus it appears, that the potash exerts its action at the expence of the entire substance of the pyroacetic spirit, and not on any oil, or other matter, contained in it.

Action of acids tried.

I tried the action of sulphuric, nitric, and muriatic acid on the pyroacetic spirit, to learn whether it were capable, like alcohol, of forming an ether.

Pyroacetic spirit with sulphuric acid,

Into two measures of the spirit, of the specific gravity of 0·8086, I poured one of moderately concentrated sulphuric acid. The mixture heated a little. It became brown immediately, and a little after very black and thick. After leaving it thus for a fortnight, I distilled it. Two fluids passed over: one colourless, and heavier: the other yellow, in smaller quantity, and floating on the former. They had a strong smell of sulphurous acid. A large quantity of coal remained in the retort. The first mixture, as well as the residuum of the distillation, was blacker, and the coal more abundant, then when alcohol is treated in the same manner.

nitric acid,

Two measures of the same pyroacetic spirit, and one measure of concentrated nitric acid, assumed a very bright and fine yellow colour, like a solution of gold, but grew darker in the course of a fortnight. I afterward distilled this mixture. At the bottom of the liquor was formed a drop of yellow oil, having the appearance of phosphorus melted in water. Nitrous gas was evolved, and this drop disappeared. A liquid passed over, that had a strong smell of nitric acid. This product I saturated with potash, and the spirituous liquor I separated by distillation. It had peculiar characters, which the smallness of its quantity did not allow me to examine minutely. In the residuum I found nitrate and acetate of potash. Oxalic acid may be obtained from the carbonaceous matter, that remains in the retort after the first distillation of pyroacetic spirit with nitric acid.

muriatic acid,

I distilled one measure of pyroacetic spirit with two of fuming muriatic acid. The liquor in the retort became brown; and, as it passed over, it was wholly condensed in the receiver. It had a strong smell of muriatic acid, and reddened blue vegetable colours. I redistilled it from carbonate of potash: when it acquired a strong smell of turpentine, and a sharp and oily taste. It was far less volatile than muriatic ether. Potash discovered no muriatic acid in it; but when burned on a solution of nitrate of silver, a very copious precipitation of muriate of silver was produced.

and muriatic acid gas.

To form this combination in a more advantageous manner, I passed a stream of muriatic acid gas through pyroacetic spirit for a considerable time. It became very brown. I distilled it from carbonate of potash, and two fluids passed over; one perfectly clear and colourless; the other lighter, with a tinge of yellow. The smell of the latter resembled that of pyroacetic spirit, but it was more fragrant. It had a hotter and more oily taste. It mixes but in very small quantity with pyroacetic spirit. It requires about forty parts of water to dissolve it. It shows no mark of acidity, and no test discovers the presence of muriatic acid in it; yet, by burning it on a solution of nitrate of silver, a very copious precipitate of muriate of silver is formed.

The spirit compared with alcohol and essential oil.

Alcohol, pyroacetic spirit, and oil of turpentine, have the property of forming combinations with muriatic acid, but each gives a result of a different nature. That of the pyroacetic spirit with this acid is neither an ether, nor a substance resembling camphor.

Is it a simple or compound product?

But are we to consider the pyroacetic spirit as a simple vegetable product, with respect to its immediate composition, as common alcohol is for instance? or as a combination of a vegetable substance with some other matter, as certain ethers, or a solution of oil in ardent spirit? Destructive distillation cannot give us certainty on this head; since from a mixture of all the substances of the vegetable kingdom we ultimately derive the same products in this way. I have made a great many experiments, in order to resolve it into other immediate principles; but hitherto it has appeared to me simple, in the sense commonly given to this word with regard to the vegetable kingdom.

Farther examination for prussic acid.

The smell of bitter almonds, which, in combination with others, characterises this substance, led me to suspect in it the presence of prussic acid; but as analysis failed to detect it, I had recourse to synthetical means.

Prussic acid combined with alcohol.

I passed a stream of prussic acid gas into alcohol, till it was supersaturated, and then distilled the mixture. It retained a strong smell of prussic acid. A second time I distilled it from carbonate of potash. The smell was nearly the same. I added carbonate of potash with black oxide of iron, and, at the expiration of a few days, distilled again. The smell of prussic acid was a little diminished. I then dissolved caustic potash in it, and distilled again. The smell of the liquor condensed in the receiver was still that of prussic acid, but somewhat modified, and participated a little in that of animal matter. The taste was sweet, but pungent. Its specific gravity was 0 8228.....

Its properties differed from those of pyroacetic spirit; and convinced me, that prussic acid combines with alcohol.

Pyroacetic spirit related to alcohol, ethers, and essential oils.

The pyroacetic spirit has some properties, that distinguish it from alcohol, ethers, and volatile oils; and others common to it and these substances. It cannot be classed absolutely with either, though it is in some respects related to each.

Contains more carbon than alcohol.

From the action of potash and acids, it would seem, that a larger proportion of carbon in its ultimate composition distinguishes it chiefly from alcohol.

A complete analysis of this substance is wanting, to make us better acquainted with it. This I am proceeding to attempt, in order to complete the series of researches, which I proposed to myself for ascertaining its intimate nature.

IV.

On raising Grass Seeds, and preparing Meadow Land: by Mr. WILLIAM SALISBURY, *of the Botanic Gardens, Brompton and Sloane Street* *.

SIR,

Grass seeds cultivated to a considerable extent.

I OBSERVE in a list of premiums offered by the Society of Arts, &c., which fell into my hands, that the subject of select grass seeds has engaged their attention. I have, therefore, taken the liberty of sending herewith a sample of festuca pratensis, or meadow fescue grass, which has been grown under my directions; it is a sample of upwards of twenty quarters, the produce of twelve acres of land, which have now been under that crop for the three last years.

The poa not adapted to general cultivation.

As the cultivation of grasses, in general, has engaged my attention very particularly for the last twenty years, I have had an opportunity of observing more attentively the several qualities, and must beg leave to observe, that the opinion formed by Mr. Curtis and others of the two species of *poa* have been proved to be erroneous in several respects; and which it will be highly proper hereafter to notice, as I have found, after numerous experiments, that neither of them is likely ever to be brought into general cultivation, for several reasons which I have fully ascertained from facts. If the mode I have practised of managing the *fescue*, *foxtail*, and other grasses, whether with the view of producing seed, or the best mode of using them in forming pasture and meadow-land, be deserving the notice of the Society, I shall be happy at a future period to send it to you.

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* Trans. of the Society of Arts, vol. xxvii, p. 67. The silver medal was voted to Mr. Salisbury for this communication.

Meadows of excellent quality speedily formed.

I beg leave to mention, that there are several meadows at Roehampton, the property of the late Benjamin Goldsmid Esq., which have been made at different times under my management with the grass seed I had previously raised, and which will challenge any other grass land in similar situations in the kingdom. The turf and pasturage of them were formed in less time by far than is to be done by the usual modes practised.

I am, most respectfully, Sir,

Your very humble servant,

WILLIAM SALISBURY.

Certificates were received, dated March 5, 1809, from William Underell, Richard Hook, and Thomas Hook, stating, that in the year 1808 there was grown upon Lee-place farm, near Godstone, in Surry, in the occupation of Mr. Pennington, one hundred and sixty-five bushels of meadow fescue grass seed, which was produced from about twelve acres of land, and that it was free from mixture of other grasses or weeds, and that they assisted in harvesting and threshing the whole.

Observations, in Addition to the preceding Statement; on the Method of laying down Pasture and Meadow Land, with an Account of some Pastures made with the Festuca Pratensis, Lin., or Meadow Fescue-Grass, and Clovers, by Mr. WILLIAM SALISBURY.

DEAR SIR,

Ray-grass inferior to many other kinds.

IT is now nearly thirty years since my much respected friend and partner, Mr. Curtis, wrote his observations on several of our native grasses, better adapted to the purposes of pasture than ray-grass, the only species of these various tribes that was then, or even now, generally used for the purpose; yet it is acknowledged, by all persons conversant on this subject, that it is inferior to many others, both in produce and nutriment, and also that it remains in the ground but a short time, a fact which attaches also to all the clovers, and is to be lamented by the grazier.

Others not cultivated to advantage from erroneous practice,

I have been often more mortified than surprised to find, that, after so much has been written on this interesting subject, and when other grass seeds may be obtained, almost every person is of opinion, that they cannot be cultivated to advantage. I am ready to admit much truth in this, when they are sown according to the common system in practice, which I have frequently convinced myself and others to be erroneous, and that it prevents these useful plants from succeeding after the seeds have been sown.

in laying land down to grass under a crop of spring corn.

It will readily be supposed, that I allude to the mode of laying down land to grass under a crop of spring corn; and I am fully sensible, that many persons will say, that it would be madness to sacrifice the benefit of a crop of barley or oats, where the land is in fine order, and while we can have a crop of grass under it.

To this I reply, that there is no land whatever, when left for a few months in a state of rest, but will produce naturally some kind of herbage, good and bad; and thus we find the industry of man excited, and the application of the hoe and the weeder continually among all our crops, this being essential to their welfare. I cannot help, therefore, observing, how extremely absurd it is to endeavour to form clean and good pasturage under a crop, that gives as much protection to every noxious weed as to the young grass itself*. Weeds are of two descriptions, and each require a very different mode of extermination. Thus if annual, as the charlock and poppy, they will flower among the corn, and the seeds will ripen and drop before harvest, and be ready to vegetate as soon as the corn is removed; and if perennial, as thistles, docks, couch-grass, and a long tribe of others in this way, well known to the farmer, they will be found to take such firm possession of the ground, that they will not be got rid of without great trouble and expense.

Extermination of weeds.

Advantage of the corn crop overbalanced.

Although the crop of corn thus obtained is valuable, yet, when a good and permanent meadow is wanted, and when all the strength of the land is required to nurture the young grass thus robbed and injured, the proprietor is often at considerable expense the second year for manure; which, taking into consideration the trouble and disadvantage attending it, more than counterbalances the profit of the corn crop.

Formation of permanent meadows.

To accomplish fully the formation of permanent meadows three things are necessary, namely, to clean the land, to procure good and perfect seeds adapted to the nature of the soil, and to keep the crop clean by eradicating all the weeds, till the grases have grown sufficiently to prevent the introduction of other plants. The first of these matters is known to every good farmer,—the second may be obtained,—and the third may be accomplished by practising the modes in which I have succeeded at a small comparative expense and trouble, and which is instanced in a meadow immediately fronting Brompton Crescent, the property of Angus Macdonald Esq., which land was very greatly encumbered with noxious weeds of all kinds, but, by the following plan, the grasses were encouraged to grow up to the exclusion of all other plants, and, though it has been laid down six years, the pasturage is now at least equal to any in the county.

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* I do not wish it to be understood, that I allude to the system, practised in Surry and other counties, of sowing clover and rye-grass under barley, when it is intended only for a season or two, in order to change the course of crop, and to be returned again to arable crops as soon as the clover is exhausted, or, what is more frequently the case, overpowered with the weeds that have been nurtured with it; this is totally different, and is not intended for a permanent crop.

Method of laying down land in grass.

Grass seeds may be sown with equal advantage both in spring and autumn; the land above-mentioned was sown in the latter end of August, and the seed made use of was one bushel of meadow fescue, and one of meadow-foxtail grass, with a mixture of fifteen pounds of white clover and trefoil; the land was previously cleaned as far as possible with the plough and harrows, and the seeds sown and covered in the usual way. In the month of October following, a prodigious crop of annual weeds of many kinds had grown up, were in bloom, and covered the ground and the sown grasses; the whole was then mowed and carried off the land, and by this management all the annual weeds were at once destroyed, as they will not spring again if cut down when in bloom. Thus while the stalks and roots of the annual weeds were decaying, the sown grasses were getting strength during the fine weather; and what few perennial weeds were amongst them were pulled up by hand in their young state. The whole land was repeatedly rolled to prevent the worms and frost from throwing the plants out of the ground; and in the following spring it was grazed till the latter end of March, when it was left for hay, and has ever since continued a good field of grass.

Grass seeds sown in the spring,

The meadows at Roehampton, belonging to the late B. Goldsmid, Esq., were laid down with two bushels of meadow fescue-grass, and fifteen pounds of mixed clover, and sown in the spring along with one peck and a half of barley, intended as a shade to the young grasses; the crop was thus suffered to grow till the latter end of June, and then the corn, with the weeds, were mowed and carried off the land; the ground was then rolled, and at the end of July the grasses were so much grown as to admit good grazing for sheep, which were kept thereon for several weeks. It should be observed, that the corn is to be mowed while in bloom, and when there is an appearance of, or immediately after rain, which will be an advantage to the grasses, and occasion them to thrive greatly.

and autumn.

I sowed some fields for the same gentleman in autumn in the same way, and found them to succeed equally well.

A history of plants used in agriculture preparing.

I intended to have made some remarks on some properties, which had escaped Mr. Curtis's notice in his observations, and which do not add to the celebrity of all the grasses he has mentioned; but as I have partly prepared a short history of the nature of all our plants used in agriculture, so far as relates to their properties in a wild state, and the effect of cultivation upon them, I shall, for the present, defer any farther remarks thereon.

I remain, dear Sir,
Your very obedient servant,
WILLIAM SALISBURY.

V.

On a Method of examining the Divisions of astronomical Instruments. By the Rev. WILLIAM LAX, *A. M. F. R. S.* LOWNDES's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* MASKELYNE, *F. R. S. Astronomer Royal*[*].

DEAR SIR,

St. Ibbs, Aug. 27, 1808.

Importance of some mode of ascertaining the accuracy of an instrument.

I AM persuaded that you must feel, in common with myself, how unpleasant it is to make use of an instrument in astronomical observations requiring extreme accuracy, the exactness of which you have no adequate means of ascertaining, but are obliged to depend for it in a great measure

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[*] Philos. Transact. for 1809, p. 232.

sure upon the abilities and integrity of the artist. It is in vain that we observe with so much nicety, and read off with so much precision, if we are still uncertain, whether there may not be an errour in the instrument itself of much greater magnitude, than those which we are endeavouring to prevent; and that our best instruments must be liable to such errours, no person can possibly doubt, who has paid due attention to the sources whence they may arise. I have estimated, as accurately as I could, the amount to which they may accumulate in Bird's method of dividing by continual bisections; and have satisfied myself, that they are much more considerable, than is generally apprehended: but as I cannot obtain such precise information as I could wish, respecting the exactness with which a bisection can be performed, or a length taken from the scale of equal parts and laid upon the instrument, I will not trouble you with the deduction which I have made. It is understood indeed, that Bird's method is now generally laid aside, and that each artist employs one, which he considers in many respects as peculiar to himself: but I presumed, that there would still be such a connection betwixt Bird's method and those which have been substituted in its stead, as to render them in some degree liable to the same errours to which it was subject; and the reports which I have uniformly received from persons, who have had an opportunity of examining some of the modern instruments, have fully convinced me, that my opinion was just. But whatever may be the nature of the methods which are now in use, or whatever their advantages over Bird's, I never could persuade myself, that it would be safe to trust to an instrument, without a previous examination. To discover the means of accomplishing this object, is what I have for some time been anxious to effect, and though I fear my endeavours have not been very successful, I will nevertheless take the liberty of presenting you with the result.

Errours in Bird's method may accumulate more than is generally supposed.

Other methods in some degrees liable to the same.

You are aware, I believe, that I use a circular instrument for observing both in altitude and azimuth, which was made for me by Mr. Cary in the Strand; that the radius of both the altitude and the azimuth circle is one foot, and that each is divided into parts containing ten minutes. The construction

Altitude and azimuth circle of one foot radius.

struction of this instrument does not differ materially from that of other similar instruments, with which you are well acquainted, and I shall not therefore waste your time by giving you a particular description of it. For the purpose of examining the divisions upon the two circles, I procured an apparatus to be prepared by Mr. Cary, which will be very easily explained. To the face of the rim which surrounds the azimuth circle, and with its left end close to the stand which supports the micrometer on the east side, an arc of brass, concentric with the circle itself, and a little more than 90° in length, an inch in breadth, and one eighth of an inch in thickness, is firmly fixed by screws, so as to have the plane parallel to the plane of the circle, and a small portion of its lower surface resting upon the extreme part of the rim. The screws pass through a brass arc, which is fastened to this at right angles, and lies with its broad side against the face of the rim. Upon the first mentioned arc, a strong upright piece of brass, about six inches in length, is made to slide, the lower part of it embracing the arc as a groove, and having a clamping screw underneath, for the purpose of fixing it firmly to the arc at any point required. To the top of the upright piece of brass is attached a microscope, with a movable wire in its focus, pointing down to the division upon the circle, not directly, however, but with an inclination to the left of about 30°. This inclination is given to it, in order to make it point to the same division upon the circle, which is immediately under the micrometer itself, when it has been moved up as near to the micrometer, as it is capable of approaching. The microscope has attached to it a small graduated circle of brass, and an index, by which the seconds, and parts of a second, moved over by the wire, are determined.

Apparatus for examining the divisions.

To the vertical circle there is likewise an arc applied, of the same length and breadth as the former, but somewhat thicker, and of a radius exceeding that of the circle by about two inches. This greater thickness is given to it, on account of its being supported in a manner which renders additional strength necessary. It is fixed with its broad convex side downwards upon two brass pillars, screwed fast to the plane of the azimuth circle, and standing in a line parallel to the plane of the vertical circle at the distance of about four inches from it, and on the right side of the pillars which support the micrometers belonging to this circle. The pillar, to which the left end of the arc is fastened, is placed close to the lower micrometer of the vertical circle, and the other contiguous to the elevated rim, in which the divisions of the azimuth circle are cut. The right end of the arc reaches beyond this pillar about ten inches. The pillars are of such a height, and so proportioned to each other, that while the left end of the arc, which lies horizontally, is raised to within about two inches of the height at which the lowest point of the vertical circle is placed, the whole arc runs parallel to the circle through an extent of something more than 90°. Upon the arc a microscope, with a movable wire in the focus, is made to slide as in the former case, and to point to the divisions upon the vertical circle, not directly, but with an inclination of about 30° to the left, in order that the same division (which is the lowest upon the circle) may be seen through it, and through the lower micrometer at the same time.

Method of examining the divisions by it.

I will now proceed to show you, in what manner the examination of the divisions upon either circle may be performed. The process is precisely the same in both cases, and will of course be described in the same words.

The first point to be examined is that of 180°, which must be done in the usual way, by bringing the points of 0 and 180° to the movable wires of the opposite micrometers; then turning the circle half-way round, and bisecting the points again with the movable wires; and lastly, taking half the difference betwixt the distances of the wires in the two positions of the circle for the errour at the point of 180°. Having now bisected the point of *zero* with the movable wire of the micrometer, which is intended to be used in the rest of the process (for we shall have no farther occasion for both), we must slide the microscope along the arc, till by moving the wire a little we can bisect the point of 90°, and then the micrometer must be firmly clamped to the arc. The circle must then be turned till the point of 180° is brought to the microscope, and that of 90° to the micrometer, so that we may be able to bisect each by a slight motion

Method of examining the divisions by it.

motion of their respective wires. This being done, we must observe, from the positions of the wires, how much the interval betwixt them has increased or decreased in the measurement of the new arc: and this increase or decrease must be noted down with a + or − accordingly. In the same manner we must proceed through the remaining two arcs of 90°, observing and noting down the difference betwixt each and the original arc.

The point of *zero* must now be brought again to the micrometer, and bisected by the movable wire, and the microscope be made to slide back along the arc, till by moving the wire a little we can bisect the point of 60°, and when this is done, the microscope must be clamped. We must then measure the arc of 60° against every succeeding arc of 60° in the circle, precisely in the same way that we measured the first arc of 90° against the other three. The arc of 45° is next to be measured against every succeeding arc of 45°, and this will complete all that is necessary to be done in the early part of the morning before the heat of the sun can have affected the temperature of the instrument. The rest may be performed at our leisure.

You will immediately perceive the object of this kind of measurement. It enables us to determine, with any degree of accuracy that may be required, the proportion which the first and every succeeding arc of the circle, contained betwixt the micrometer and the microscope, bears to the whole circle, and of course the absolute length of the arcs themselves. Let a denote the real length of the first of these, and $\pm a'$, $\pm a''$, $\pm a'''$, &c., the difference betwixt the first and second, the first and the third, &c., respectively: let A represent any other arc, the length of which is known, and which is a multiple of a, as marked upon the instrument, and let this multiple be expressed by n. Then will $a + (a + a') + (a + a'') + (a + a''') + \&c. \ldots (a + a'''\ldots\overline{n-1}) = A$, and $a = \frac{A - a' - a'' - a\cdots a'''\cdots\overline{n-1}}{n}$. Hence it is evident, that, if there is no errour committed in the measurement of any of these arcs, we shall have the value of a, and consequently of $a + a'$, $a + a''$, $a + a'''$, &c., and of any arc, comprehending

Method of examining the divisions by it.

prehending any number of these, accurately determined. But if there be an errour of e in the measurement of the first, e', e'', e''', &c., in the measurement of the second, third, &c., respectively, then we shall have the following equation for determining a, viz. $a + (a + a' + e + e') + (a + a'' + e + e') + \&c. \ldots (a + a'''\cdots\overline{n-1} + e + e'''\cdots\overline{n-1}) = A$, and consequently a will appear to be equal to

$$\frac{A - a' - a'' - \ldots a'''\cdots\overline{n-1}}{n} - \frac{\overline{n-1}\,e - e' - e'' - \ldots e'''\cdots\overline{n-1}}{n},$$

which differs from its true value by $\dfrac{\overline{n-1}e + e' + e'' + \ldots e'''\cdots\overline{n-1}}{n}$. Hence it follows, that the value of the p^{th} arc (p being greater than unity,) as deduced by this process, will differ from its true value by $\dfrac{\overline{n-1}.e + e' + e'' + \ldots e'''\cdots\overline{p-1} + e'''\cdots p + \ldots e'''\cdots\overline{n-1}}{n} - e - e'''\cdots\overline{p-1}$; and that, if we add any number p of these arcs together, in order to determine the value of the arc which is equal to their sum, we shall have an errour in this value (and the expression holds when p is unity, or the first arc only is taken) equal to p

$$\frac{\overline{n-1}.e + e' + e'' + \ldots e'''\cdots\overline{p-1}}{n}\ \frac{+ e'''\cdots p + \ldots e'''\cdots\overline{n-1}}{n}$$

$$-\overline{p-1}\cdot e - e' - e'' - \ldots e'''\cdots\overline{p-1} = \frac{\overline{n-p}\cdot e - e' - e'' - \ldots e'''\cdots\overline{p-1} + p\,.\,e'''\cdots p + e'''\cdots\overline{p+1} + \ldots e'''\cdots\overline{n-1}}{n}.$$

Now, if we suppose e to be the greatest errour to which we are liable in the measurement of any arc, and each of the succeeding errours to be equal to it, and likewise that e', e'',…$e'''\cdots\overline{p-1}$ are all negative; then it will appear, that $\dfrac{n-p}{n} \times 2pe$ will be the greatest errour that can be committed in determining the value of any arc, by adding together the values of the (p) smaller arcs of which it is compounded. For instance, if the interval betwixt the micrometer

Method of examining the divisions by it.

micrometer and the microscope comprehends an arc of 60°, as marked upon the instrument, and this arc is measured against every succeeding arc of 60° in the whole circle, we shall have the greatest errour that can be committed in deducing the arc of 120° from the addition of the two first arcs of 60°, equal to $\frac{6-2}{6} \times 2 \times 2e = 2.66e$. After these remarks, we may proceed to consider how the remaining divisions upon the circle may be examined with the least probable errour, and to ascertain the amount of the greatest to which the process can in any case be liable.

Let the arc of 30° be now measured against every succeeding arc of 30° in the first, third, fourth, and sixth arcs of 60°; and let the length of each be determined from a separate comparison with the arc of 60°, in which it is comprehended, and not from a general comparison with all the four. The arc of 15° must then be measured against every succeeding arc of 15° in the first, third, fourth, sixth, seventh, ninth, tenth, and twelfth arcs of 30°, and the value of each deduced from a comparison with the arc of 30, in which it is contained. When this is done, we shall have determined the length of every succeeding arc of 15°, of the first arcs of 30°, 45°, 60°, 75° (= 60° + 15°) 90°, 105° (= 90 + 15°), 120° (= 60 + 60°), 135° (= 90° + 45°), 150° (= 120° + 30°), 165° (= 150° + 15°), and 180° in each semicircle.

We must next measure the arc of 5° against every succeeding arc of 5° in the whole circle, and deduce the values of the first, and of the sum of the first and second, in each succeeding arc of 15° from a comparison with the arc of 15° in which they are contained. We must then proceed to determine the values of the first arc of 3° in each 15°, and of its multiples the arcs of 6°, 9°, and 12°. We must also put down the value of the last arc of 3° in each arc of 15°, and then deduce the values of the first and last arcs of 1° in each arch of 15°, from a comparison with the arc of 3° in which they are respectively contained.

We shall now have measured in each arc of 15° the first arcs of 1°, 3°, 5°, 6°, 9°, 10°, 12°; and by taking the last arc of one degree, which has likewise been determined, from

Method of examining the divisions by it.

from the arc of 15°, we shall obtain the first arc of 14°. The first 7° of this arc being measured against the second, we ascertain the value of the first 7°; and then, by measuring the first 4° of the remaining arc of 8° against the second, we shall get the value of the first 4°, which added to the arc of 7°, before determined, will give us the length of the first arc of 11°. The first 2° of the remaining arc of 4° must then be measured against the second, and we shall get the value of the first 2°; and by adding this arc to the arc of 11°, we shall obtain the value of the arc of 13°. By taking away the first arc of 1° from the arc of 15°, we get the remaining arc of 14°, and then having determined the length of the first 7° of this arc, by measuring them against the second, we must add it to the arc of 1°, and we shall obtain the arc of 8°. The length of the first 4° of this arc will then be easily known, by measuring them against the second, as will afterward that of the first 2° in the arc of 4° itself, by measuring them against the second in the same arc.

We have still to ascertain the lengths of all the first arcs of 10, 20, 30, 40, and 50 minutes contained in each degree, for I shall only consider the case in which the circle is divided into parts of 10 minutes. Now the length of the first arc of 30′ will be obtained by measuring it against the second; and the lengths of the first and second arcs of 20′ (the sum of which will give the arc of 40′) by measuring the first against each of the remaining arcs. The length of the third arc of 20′ must likewise be put down; and then the first arc of 10′ being measured against the second of the arc of 20′, in which it is included, and also against the two arcs of 10′ contained in the last arc of 20′, its own value, and that of the last 10′ in the degree will be determined from a comparison with the arcs of 20′, in which they are respectively comprehended. The length of this last arc of 10′ being taken from that of the whole degree will give us the length of the first 50′, and complete the operation.

Greatest possible errour.

In order to ascertain the greatest possible errour to which we are liable in the examination, let ϵ denote in parts of a second the greatest that can be committed in bisecting any point upon the limb; then, since this errour may occur at each end of the arc, it is evident, that e in the expression deduced above $\left(\frac{n-p}{n}\times 2pe\right)$ will become 2ϵ, and the expression itself $\frac{n-p}{n}\times 4pe$. Hence the possible errour will be $\frac{2-1}{2}\,4\epsilon = 2\epsilon$ at 180°; $\frac{2\epsilon}{2}+\frac{2-1}{2}\times 4\epsilon = 3\epsilon$ at 90°; $\frac{2\epsilon}{3}+\frac{3-1}{3}\times 4\epsilon = 3{\cdot}33\epsilon$ at 60°; $\frac{2}{3}\times 2\epsilon+\frac{3-2}{3}\times 4\times 2\epsilon = 4\epsilon$ at 120°. The greatest errour must therefore lie betwixt 90° and 120°, and nearer to the extremity of the latter than the former arc. At 105° it will be $5{\cdot}50\epsilon$; at 111° it will be $5{\cdot}50\epsilon - \frac{2}{5}.1{\cdot}5\epsilon+\frac{5-2}{5}\times 4\times 2\epsilon = 9{\cdot}70\epsilon$; and at 111° 10′ it will be $9{\cdot}70\epsilon - \frac{1}{6}.1{\cdot}04\epsilon$ (the excess of the errour at 111° above that at 112°) $+\ 3{\cdot}33\epsilon = 12{\cdot}86\epsilon$, which will be found to be the greatest errour betwixt 105° and 120°, and of course the greatest in the first semicircle. In the other semicircle, the process being the same, the possible errours must necessarily be the same at the same distances from the first point, reckoning the contrary way upon the circle.

The magnitude of the quantity ϵ will of course vary upon circles of the same radius, according to the excellence of the glass employed, and the accuracy of the examiner's eye. It will seldom, however, exceed one second upon a circle, the radius of which is one foot; and in general it will not amount to so much. I find that I can read off, to a certainty, within less than three fourths of a second, and hence I conclude, that I could examine the divisions of my circle without being liable to a greater errour than 9·63 seconds, and those of a circle of three feet radius without the risk of a greater errour than 3·21 seconds.

This inconsiderable compared with those incident to dividing,

To those people who are accustomed to entertain such exalted notions of the accuracy, with which astronomical instruments can with a certainty be divided, this errour, I dare say, will appear very considerable; but for my part, I am perfectly satisfied, that it bears but a small proportion to the accumulated errour which may take place, in spite of the utmost vigilance of the artist, in an instrument divided according to any method which has hitherto been made public. I need not, however, remark upon the very great improbability, that the errour of examination should ever attain, or approach, to its extreme limit, as this must be sufficiently obvious to any person, who is in the least degree conversant with the doctrine of chances; but it may be proper to observe, that we have it in our power (and in this respect the examiner possesses a most important advantage over the divider of an instrument) to diminish its probable amount, as much as we please, by bringing the movable wires of the micrometer and microscope several times to bisect their respective points in the measurement of every arc, and taking a mean of the different *readings off* for the true position of the wire at the real bisection of the point. The wire may be moved in this manner eight or ten times at each point (if such a degree of caution should be thought necessary,) and the mean taken in little more than a minute, so that the time of performing the work will not be so much increased, as might perhaps have been apprehended; and when it is completed, we may reasonably presume, that the distance of every point from zero (while the temperature of the circle continues uniform) will have been determined with sufficient exactness for every practical purpose.

will scarcely ever occur,

and may be greatly diminished.

Of the time necessary for the examination a pretty correct idea may be formed, by considering how many measurements are required, and allowing about a minute and a half for each; *i. e.* a quarter of a minute for bringing the extreme points of the arc to the micrometer and the microscope, and a minute and a quarter for making the several bisections. Now, in dividing the whole circle into arcs of 15° each, it will appear, that forty-four measurements must be performed; and to examine every point in each arc of 15°, there will be 161 required, making in all 3908 measurements; and consequently the time necessary for completing the whole work will be 5862 minutes, or about 98 hours.

Time required for the examination.

The time and labour required for this examination are, no doubt, very considerable; but it ought to be recollected, that it will render any great degree of precision, in dividing the

It renders great precision in an instrument unnecessary;

the instrument, totally unnecessary. Whoever indeed employs this method of examination will be virtually the divider of his own instrument; and all that he will ask of the artist is, to make him a point about the end of every five or ten minutes, the distance of which from zero he will determine for himself, and enter in his book to be referred to when wanted. We may likewise observe, that by this examination we shall not only be secured against the errours of division, but against those which arise from bad centring, and from the imperfect figure of the circle, and which in general are of too great a magnitude to be neglected.

counteracts bad centring and imperfection of figure;

It will, I dare say, have occurred to you, that, whenever we are desirous that an observation should be particularly exact, we may guard it against the effects of unequal expansion or contraction in the metal, by means of the apparatus which I have described: for we have only to measure the arc which has been determined by the observation against the whole circle, or against the multiple of it, which approaches nearest to the circle, and thence to deduce its value in the manner explained above; and we shall either have entirely excluded the errour which we apprehended, or have rendered it too small to be of any importance. Suppose, for instance, that the arc determined by the observation was 48°; then by measuring it against the whole circumference increased by an arc of 24°, we shall obtain a result free from any greater errour of unequal temperature, than one eighth of the increase or decrease of this arc of 24° beyond a due proportion to that of the circle itself.

may be employed to obviate errours from inequality of temperature;

This expedient gives us all the advantages of the French circle of repetition, without the inconvenience arising from being obliged to turn the instrument, and move the telescope, so many times in the course of the observation. Nay, I am persuaded, that the result may be made more accurate in this way, than by the French method; because not only can the object be more frequently observed, but the contacts or bisections, it may be presumed, will be more exact, when the observer is not disturbed by the hurry attendant upon the use of the repeating circle; and with respect to any errour in the instrument, from whatever cause it may arise, it will be as effectually excluded by the process which I re-

gives all the advantages of the French circle of repetition without its inconveniences, and is even superior to it;

I recommend, as by moving the telescope round the circle. Besides, this method is applicable either to the azimuth or altitude circle, or indeed to any circle which turns upon its own axis, whereas the French method can never be applied to the azimuth circle, or to any other circle, which does not turn both upon its own axis, and upon one which is perpendicular to it.

and is applicable to all circles turning upon their own axis, which the French method is not.

After all, however, it is possible, that the process which I have been explaining to you may be no new discovery, and that you may be already acquainted with it. If this should be the case, you will be kind enough to inform me. At any rate, indeed, I should esteem myself greatly obliged, if you would favour me with your sentiments upon the subject, as soon as you can do it with perfect convenience to yourself.

I am, Dear Sir,
Yours, &c.
WILLIAM LAX.

VI.

A Physical View of the Equatorial Regions, drawn up from Measures taken and Observations made on the Spot, from the tenth Degree of North to the tenth of South Latitude, between the Years 1798 *and* 1804, *by Mr.* von Humboldt *.

THE author has here collected together the natural phenomena of the equinoctical regions, from the level of the Pacific Ocean to the highest peak of the Andes. The particulars, from which these results are given, will be found in his travels at large; but this connected view of their principal features cannot fail to be highly interesting to the natural philosopher. Mr. von Humboldt has not extended this summary nearer the tropics than the latitude of 10°, on account of the great difference observed, not only in the productions of the soil, but more particularly in the meteorological phenomena, between 10° and 23°.

Natural phenomena in the vicinity of the equator.

These vary greatly on approaching the equinox.

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* Magazin Encyclopédique, Sept. 1807, p. 139.

From

Plain on the west. From the western shore of America, in this region, to the chain of the Cordilleras, we find a plain stretching a considerable distance from north to south, but in no place exceeding twenty or thirty leagues in breadth. Chimborazo. Chimborazo, the loftiest summit of the vast chain of the Andes, rises in Its height. 1° 27′ south latitude. From the author's barometrical measurements, calculated by the formula of Mr. Laplace, this mountain is 6536 met. [7143 yards] above the level of the sea. Mr. Condamine gives 180 met. [197 yards] less: a difference naturally arising from his neglecting the temperature.

Vast volcano. In the map accompanying this volume, where Mr. von H. has given a vertical plan of Chimborazo, the summit of Cotopaxi appears behind it; a volcano nearly five times as high as Vesuvius, and the roarings of which were heard in 1744 to the distance of 220 leagues.

The east face of the Andes steepest. Several eminent natural philosophers have supposed, that the western slope of the Andes was much steeper than that on the east: but Mr. von Humboldt has found the contrary to be the case, after traversing the chain in several directions.

Geography of plants. The part which the author has dedicated to the geography of plants exhibits many new views. On the plain of Chimborazo he has inserted the names of the plants growing on it at different heights, which he determined accurately by means of the barometer. His comparison of them with the plants growing at the same heights on the Alps and Pyrenees is extremely curious: and he has noted the height they attain, according to the different situations and elevations where they grow.

Variation of temperature. This article is followed by a series of observations on every thing that varies with the heights to which you ascend above the level of the sea. The table of variations of temperature exhibits the maximum and minimum of heat, which the centigrade thermometer indicates from five metres to five hundred. Cold of the Andes. It appears, that the cold of the Andes is not very considerable; but many circumstances combine to render it difficult to bear. On Chimborazo, at the height of 5908 met. [6457 yards], the thermometer descends only Heat near the rivers. to − 1·8° [28·76° F.]. In the hottest regions on the banks of the Amazon, Magdalena, and other rivers, the mean temperature is 27° [80·6° F.]; but the thermometer seldom rises to the extremes of heat, which it frequently attains in the north of Europe. In the equatorial regions the extremes of the greatest and least heat are not more than 16° or 20° [28·8° or 36°] distant; while in Europe they exceed 62° [81·6°].

Extremes of heat and cold.

From all the observations made by Mr. von H. on the Cordilleras it appears, that the decrease of temperature above 3500 met. [3825 yards] is more rapid in the proportion of 5 to 3, than from the level of the sea to the height of 2500 met. [2732 yards]: but it is to be remarked, that the inequalities of the surface of the Earth have much influence on this decrement; and that a person going up to the same heights in a balloon would find different results.

The temperature decreases more rapidly at great heights.

This influenced by the inequality of the surface.

Mr. von Humboldt ascended Chimborazo to such a height, that the barometer fell to 0·37717 of a metre [14·84 inches]; its height at the level of the sea being settled at 0·76202 of a met. [29·98 in.], and the temperature being 25° [77° F.]. This differs a little from the estimation of Bouguer, who made it 0·76022 of a met. [29·91 in.].

Heights ascended by the author.

The elasticity of the air in the temperate zones varies sometimes 0·045 of a met. [1·77 in.] in the same place. Under the tropics, at the seaside, it varies only 0·0026 of a met. [1·023 line]. In a separate table Mr. von H. gives these variations at the different hours, at which they are noticed. The progress of these variations under the equator, at the level of the sea, supposing the mean term of the barometer $= Z$ is as follows.

Variations of the barometer.

At 21 hours $= Z + 0·5$ At 11 hours $= Z + 0·1$
At 4 $= Z - 0·4$ At 16 $= Z - 0·2$

These horary variations are not noticed in our climates, on account of the many local causes, that occasion the barometer to rise or fall irregularly.

Mr. von H. concludes his barometrical researches with some physiological observations on the connexion between respiration and the elasticity of the air. It appears that the inhabitants of the plains, accustomed to a pressure of 28 in. [29·84 Eng.], most easily support these variations of the density of the air: they easily habituate themselves

Effect of the weight of the air on respiration.

to that of Quito, which answers to 20 in. 1 line [21·4 in. Eng.], and other places still higher, where the barometer falls to 17 in. 4 l. [18·47 in. Eng.].

Hygrometrical observations. In his hygrometrical observations Mr. von H. sometimes used Saussure's hygrometer, sometimes Mr. De Luc's; but all his results were reduced to the degrees of Saussure's, correcting them for temperature.

Moisture on the summit of the Andes. On the summit of the Andes, where the hygrometer falls to 31·7°, there still remains great moisture; and the freshness retained by the vegetation sufficiently proves, that it derives from aqueous vapour the aliment, that supports it amid extreme drought.

Height of clouds. The aqueous vapour, which exhibits itself in large masses, seems to maintain nearly a constant height. From the measures of Mr. von H. the lower stratum appears to support itself at 1169 met. [1277 yards], and the higher at 3300 [3606 yards] above the level of the sea. As to those little clouds, which are vulgarly termed flocks [*moutons*], it is very remarkable, that they commonly rise to the height of more than 7800 met. [8524 yards].

Quantity of rain. Mr. von H. estimates the quantity of rain, that falls annually between the tropics, at more than 1·89 met. [74·36 in.], while in Europe it is only 0·48 [18·88 in.*].

Electricity of the atmosphere. The electrometrical table exhibits several equally curious results. The electric intensity increases considerably as you approach the summit of the Cordilleras. The equatorial regions from the sea to the height of 2000 met. [2186 yards] are but slightly charged with the electric fluid; it accumulates in the clouds however, and there causes frequent explosions. They recur periodically, in general two hours after the culmination of the sun; and their violence in these climates is well known. Hail. At the height of 3000 met. [3278 yards] a great deal of hail is formed; and the atmosphere at this height is commonly charged with negative electricity.

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* This is certainly much too small. A mean for 11 different places in England for 1809, see Journal, XXV, p. 309, gives 31·15: this is near Mr. Dalton's mean for all England, taking first a mean of the counties: and a mean of several places on the continent, as well as in England, from the table in Dr. Young's Natural Philosophy, vol. II, p. 477, gives 32·1. C.

Blueness of the sky.

It is well known, that the blueness of the sky is deeper in proportion as the air is more dilated. Mr. von H. thought he observed, that in general it was more intense under the tropics, than at an equal height in Europe. This he ascribes to the complete solution of the vapours in the equatorial atmosphere. On the Andes the blueness was 46° of Saussure's cyanometer.

Light.

The decrease of light is greater in proportion to the density of the strata of the air; accordingly it is much less on the summits of high mountains. In general the light is much stronger under the tropics, than at equal heights in Europe. This is confirmed by the light reflected from the moon to the Earth in a total eclipse. This intensity of the light probably acts on vegetable productions, and contributes to that resinous and aromatic character, which they exhibit on the tops of mountains. Perhaps too it exerts its influence on the nerves; as the inhabitants of Quito have a sensation of faintness, whenever the sun darts his rays on them.

Atmosphere.

Bouguer calculated, that the atmosphere, reduced to the temperature of 0° [32° F.] throughout its whole extent, and to a column of mercury of 0·76 met. [29·9 in.], would be 7820 met. [8546 yards, or not quite 5 miles]: the observation of the twilight shows, that at 60000 met. [65571 yards, or rather more than 37 miles] the air has sufficient density to send us a perceptible light.

Uniformity of its composition.

Mr. von H. gives some results of experiments made by him and Mr. Gay-Lussac on the chemical composition of the atmosphere. From these experiments the component parts of the air are 0·210 of oxigen gas, 0·787 of nitrogen, and 0·003 of carbonic gas. If there be any variation, it appears not to exceed a thousandth part of oxigen; and Mr. Gay-Lussac found it the same at the height of 7000 met. [7650 yards].

Of the interesting series of geological observations we can give here but a few particulars.

The

Mountains.

The height of the loftiest mountains is so trifling compared with the radius of the Earth, that it has had very little influence on the grand phenomena of geology: but it is perceptible with regard to a small part of the Earth's surface, and the law according to which the different rock-formations are arranged above the level of the sea. The equatorial regions exhibit both the loftiest summits, and the most extensive plains. We cannot therefore ascribe to the rotary motion of the globe this group of mountains. Besides, the chain of the Andes at its two extremities extends toward the poles as far as 29° or 30° *. This chain is very unequal: in many places it is not above 200 met. [218·57 yards] high; but in four places it attains a colossal height. These are in 17° south latitude; under the equator in the kingdom of Quito; at Mexico, in the latitude of 19° north; and opposite Asia, in 60° north lat. This height varies from 5000 to 6000 met. [5464 to 6557 yards]. The chain of the Andes is not less astonishing for its bulk, than for its height. Near the volcano of Antisana, at the height of 4000 met. [4371 yards], Mr. von H. found a plain twelve leagues in circumference. In some parts this chain is from 40 to 60 leagues broad.

Chimborazo forms one extremity of a colossal group. The chain that extends to the south stretches so far toward the ocean, that the islands near the continent may be considered as fragments of it. On the north the Cordillera separates into three branches. The easternmost forms the group of Santa Martha and Merida. The westernmost, from which we are furnished with platina, forms a group near the city of Mexico, some of the peaks of which, as Popocatepec and Oribaza, exceed 5300 met. [5792 yards]. This chain afterward diminishes considerably, but in the vicinity of Asia it forms a fourth group, in which Mount St. Elias is 5512 met. [6024 yards] high. In these latitudes the Andes appear to have a subterranean communication with the volcanoes of Kamtschatka.

Interior structure of the

In their interior structure the mountains of the equator unite almost every kind of rock discovered in the rest of

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* It should be 50° or 60°. C.

the

equatorial mountains.

the globe. The arrangement of their strata too is perfectly similar. Granite forms the base of the chain of the Andes; and their ridge is every where covered with porphyritic formations, basaltes, phonolites, and greenstone. But a circumstance peculiar to the equatorial mountains is the vast height, to which the rocks of subsequent formation to granite ascend. This base is almost wholly covered by these more recent formations, and the highest point at which Mr. von H. observed it was 3500 met. [3825 yards]. The summits of Chimborazo, Antisana, &c., at the height of 6372 met. [6964 yards], are of porphyry. Mr. von H. likewise points out the different heights, at which the other stratifications are met with. The remains of organic bodies are very rare in the mountains near the equator, as calcareous stones are by no means abundant. Some are found however at the height of 4300 met. [4699 yards]. The fossil bones of elephants, which Mr. von H. has collected, were found about 3000 met. [3278 yards] only.

Sulphur.

The great masses of sulphur, that abound in the Cordillera, are frequently met with in primitive rocks, at a distance from volcanoes.

Volcanoes.

The greatest number of volcanoes are found in the chain of the Andes. There are more than fifty, between cape Horn and mount St. Elias, that still emit flames. Some of them pour out lava; others, as those of Quito, throw up scorified rocks, water, and clay.

The most recent volcano is that of Xorullo, or Jorullo, [of which an account has already been given in p. 81 of this volume]. The air collected at the bottom of the crater by Messrs. von H. and Bonpland contained more than 0·05 of carbonic acid.

Limit of snow.

The lowest limit of the snows under the equator is one of the most constant of the natural phenomena. From the various measures which he took Mr. von H. estimates it at 4795 met. [5240 yards]. The Cordillera of the Andes has no glaciers, owing probably to the little snow, that falls between the tropics; but at Chimborazo, on digging in the earth, snow of very great antiquity is found.

In his description of the animals that inhabit the Andes, Mr. von H. notes the different heights, at which they are seen.

Agriculture.

His last article relates to the cultivation of the soil, in the regions he has sketched. Several villages on the acclivity of the Andes are built at a considerable height. From the level of the sea to the height of 1000 met. [1093 yards] the plantain, maize, and chocolate nut are cultivated. It is the region of the most delicious fruits. The Europeans have introduced other plants; the sugar cane, indigo, and coffee tree. The last of these delights in a high and stony soil.

Wheat grows in the greatest abundance in Quito and Peru from the height of 1600 to 1900 met. [1748 to 2076 yards]. The great plains of the Cordillera are particularly favourable for the corn of Europe. The soil, yielding easily to the plow, shows that they were anciently the bottoms of lakes.

The plains bottoms of old lakes.

From 3000 to 4000 met. [3278 to 4371 yards] the principal article of culture is the potato. Higher up the people live amid their numerous flocks of lamas, which frequently wander as far as the region of snow.

VII.

Observations on the Pleonast Spinel, particularly that of the Environs of Montpellier. By Mr. Marcel de Serres *.

Synonimes of the pleonast, found in Ceylon,

THE pleonast of Hauy, brown garnet or schoerl in truncated dodecaedrons of Romé de Lisle, was originally found in the island of Ceylon, among tourmalines and other crystalline substances, with which it was confounded. Delamétherie first made a distinct species of it under the name of ceylanite. Sometime afterward he found it in rocks thrown up by Vesuvius. Mr. Lhermina subsequently observed it in the same rocks: and Mr. Lewis Cordier lately obtained it from almost all the volcanic rocks in the environs of Closterlach on the borders of the Rhine. Draparnaud mentions pleonast in the brecciæ of the little basaltic moun-

and distinguished as a species by Delamétherie.

Occurs in volcanic rocks.

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* Journal de Physique, vol. LXVII, p. 26.

taiu

tain of Montferrier: in fact it is met with there in a tufa, that has the form of a breccia. I have never yet found it in a breccia, but like those that came from Ceylon, which are most commonly in amorphous or rounded masses, that have experienced a commencement of alteration. Perhaps this word is too strong to mark the state in which this substance is found, for it appears too hard, to alter easily; yet its colour is so dull, that it appears to be altered.

An accidental mixture in brecciæ.

Apparently the pleonast is a very accidental mixture in the breccia: a few of the crystals, detached from the rocks in which they were contained, have been united to the various surrounding substances by some cement*. Crystallized pleonast rubies are found likewise in the bottom of the gullies at the foot of the little mountain of Montferrier, and almost always on the surface of the detritus of the surrounding substances.

Found in a sand near Montpellier.

A totally different situation, where this substance equally presents itself, and in pretty large quantity, is at Soret, on the left bank of the Lez, about a mile from Montpellier. It is found on the surface of a sand mixed with shells and bowldered quartz. This sand rests on strata of sandstone, and very various and heterogeneous agglomerations of the same nature. Sometimes these strata are covered by others of shelly limestone, filled chiefly with the oyster, cockle, and acorn shell. The oysters frequently contain others, which appear to have grown in their cavity. The strata of sandstone are very irregular, most frequently horizontal, and containing numerous concretions of sandstone, in the shape of pears, apples, and tears, almost always in the same position; which indicates, that these concretions were not formed in the manner of common stalactites, but as the nodules of silex. It appears evident, that the pleonasts occur in them accidentally, and were brought thither by the waters. I have since found some in the volcanic hill of

Sandstone strata.

Formation of brecciæ and sandstones.

* Secondary calcareous waters appear to have formed this breccia, and to enter into the formation of all the known brecciæ and sandstones. In fact, water loaded with carbonate of lime, flowing from the base of the basaltic prisms, has there deposited the earth with which it was impregnated, agglutinated all the broken pieces of the surrounding rocks, and thus formed a breccia.

Valmahargues, 6 kil. [3¾ miles] north of Montpellier; and likewise in a stratum of basaltic tufa 3 kil. [15 furlongs] long, at the bottom of a hill called *lou Hâout*, or *lou Nâout*, near Prades, on the north-east of Montferrier.

Is the pleonast a volcanic production?

A question, by no means uninteresting, that naturally presents itself, is, whether the pleonast ruby be a volcanic product, or not. From its hardness it might be presumed, that it is altogether foreign to the lavas, and formed in the humid way, anterior to its deposition in the strata where it is found. It may be said, that none has yet been seen in the lava of the Vivarais, Auvergne, Etna, the Lipari Islands, Iceland, or the Isle of France; but only in the cavities of some rocks of Vesuvius, Somma, Closterlach, and Campania. Hence we have sufficient reason to believe, that it belongs to the primitive rocks, and that to see it intimately united with the tourmalines of Ceylon is sufficient to convince us of this. Brongniart however is of opinion, that this mineral, as well as the telesia, or corundum, belongs to the secondary trap formation. His opinion is founded perhaps upon that of Werner; who judges from the nature of the strata, of which the sand containing corundums appears to be the remains, that those of a hardness much superior to the pleonast must belong to that formation. Thus as basaltes and basaltic tufa are rocks of the secondary trap mountains, and these are met with in the places where the pleonast is found, this opinion appears to have some probability: but as we have not yet any accurate description of the mineralogical situation of the corundums; and as the adamantine spar is found in granite rocks, entering even into their composition in the same manner as feldspar; we may consider the corundum, adamantine spar, and pleonast as belonging exclusively to the primitive formations. This opinion however can rank only as a probability, till we have a precise knowledge of the mineralogical situation of these interesting substances.

Probably it belongs to the primitive rocks,

though supposed by some to be of the secondary formation.

VIII. *On*

VIII.

On the Nodules of Lava found in the Klingstein of the Rock of Sanadoire *.

Nodules of lava in the phonolite of Sanadoire.

THE rock of Sanadoire having been described by various naturalists †, we shall confine ourselves to the mentioning of a fact, which had long escaped the notice of those who visited this singular rock, that of nodules of tumefied lava imbedded in the clinkstone. This fact was first observed by Mr. Lacoste in 1804 ‡; and it has been since noticed by Messrs. Menard and Alluaud in the summer of 1807. Having accompanied these gentlemen in their visits to the Mont d'Or, we shall point out the road we pursued, and the precise places where this fact occurs.

Places where it occurs.

1. Proceeding from the lake of Guéri to the rocks of Sanadoire and Thuilière, by the road from Mont-d'Or to Rochefort, and beginning to descend toward the valley, where these two rocks are situate, so as to see their summit; on the left of the road, and not above thirty yards from it, is a small eminence, or inequality of the ground, formed of masses and fragments of phonolite, in which nodules of lava are imbedded.

2. On ascending the great slope at the base of Sanadoire, which is on the west, and directly opposite Thuilière, we find similar nodules in the crumbled materials from above, which are all of phonolite. Some of these nodules scarcely adhere to the mass that includes them, and are separable by a single stroke of a hammer. Others adhere to it more closely, but still do not form one continuous substance with it. The surface of those separated by the hammer is rounded, but irregular, smooth, and covered with a slight shining coat, which appears to be a very thin pellicle of klingstein.

The lava of different kinds. 1st kind.

These nodules are of different kinds of lava.

A. A black, compact lava exhibiting in its fracture a multitude of little needles of amphibole, or pyroxene; so that they might be taken for fragments of a hornblende rock, if some of them did not exhibit unequivocal marks of the action of fire.

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* Journal de Physique, vol. lxvii, p. 54.

† Memoirs of the French Academy, for 1771 and 1773; Desmarets, Mémoire sur le Basalte. Journ. de Phys. tome 59, an 1804. Daubuisson. Mémoire sur la Phonolithe.

‡ Lacoste's Geological Letters, a work in which they who visit Auvergne will meet with facts no where else described.

2nd kind.

B. A porous, tumefied lava, of a gray and blackish gray ground, with needles of pyroxene, and laminæ of feldspar. This is the most common variety.

Not peculiar to this rock.

3. This fact is not peculiar to the rock Sanadoire and those in its vicinity: a large bowldered block in the torrent of Prent-Garde, coming from the lake of Guéri, exhibited to us the same phenomenon. The klingstein is a variety of that of Sanadoire, of a bluish gray colour, and less polyedral in its fracture.

3d kind.

C. The lava it contains is gray, and composed of very small crystals mingled together.

Found in another mountain,

4. We could not visit la Védrine, another phonolitic mountain on the east of Mont-d'Or, but Mr. Menard informs us, that he found there the same peculiarities as at Sanadoire, which had wholly escaped his notice at his first visit to that mountain.

and probably in several places.

It is not in the simple clinkstone, that we found these nodules of lava; perhaps they would equally be found in the porphyry with base of clinkstone of the environs of lake Guéri, Puy Gros, the rock of Dardanche, &c. By an attentive research, employing the time requisite for an examination of this interesting part of the Monts-d'Or, we have no doubt they would be found in much greater number. One single specimen has presented us with two or three nodules. Their size varies from a few cubic millimetres to fifteen cubic centimetres [from one or two cubic lines to near a cubic inch.]

Size of the nodules.

This stone therefore a lava.

This stone has always been considered as a lava in France, either under the name of greenish petrosiliceous lava, or of prismatic and tabular greenish basaltes: but its being found among volcanic substances was the sole ground of this opinion, for marks of the action of fire had no where been observed on it. The volcanic origin therefore of the rock Sanadoire, long disputed, is now evidently proved by the presence of the nodules of lava it contains, which presupposes the rock to have been in a state of fluidity.

IX.

IX.

Chemical Examination of a Substance found in Balsam of Mecca: by Mr. VAUQUELIN*.

Portion of balsam of Mecca not taken up by alcohol.

MR. HALLE, a member of the Institute, &c., requested me to examine a substance, which he obtained from balsam of Mecca on dissolving it in alcohol by means of heat. This substance had the appearance of a resin, was transparent, and had an agreeable smell. Thrown on burning coals, it emitted a smoke and smell like those of frankincense, and left no coally residuum.

Boiled repeatedly in alcohol.

I took a gramme [15½ grs.] of it, and treated it with alcohol at 40°. The first effect of this was to render it opake from the surface to the centre, as soon as it began to exert its action, and it soon gave it a flocculent form throughout. I continued to boil fresh quantities of alcohol on this flocculent substance, till it no longer rendered distilled water turbid.

$\frac{1}{3}$ insoluble.

The insoluble flocculent matter, being collected, weighed 0·3 of a gramme, or nearly a third of the substance employed. Heat united it into masses of great tenacity, which drew out into threads like birdlime, without however possessing any elasticity. Thrown on burning coals it emitted the same smell of frankincense as before, and left no residuum.

The solution evaporated.

The alcoholic solution was transparent while hot; but on cooling grew turbid, though it let fall no sediment. During evaporation a white flocculent matter appeared, in proportion as the alcohol was diminished; and when completely dried part remained in a pulverulent and spongy form. Another portion united into a transparent mass, having nearly the appearance of turpentine. Both of these burned like resins, but with a less agreeable smell than the part insoluble in alcohol.

It consists therefore of two substances,

These experiments prove, that in the residuum of balsam of Mecca insoluble in cold alcohol, put into my hands by Mr. Halle, there are two substances; one, which dissolves in a very large quantity of boiling alcohol; and another, which does not combine with it at all, though of a resinous nature.

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* Annales de Chimie, vol. lxix, p. 221.

either employed to sophisticate it, or natural to it;

Do these two substances exist at the same time with the balsam of Mecca in the tree that furnishes it, the amyris opobalsamum? are they formed at the expense of the balsam, by a change effected by keeping? or are they fraudulently mixed with the balsam?

but probably the latter.

To solve these questions it would be necessary, to examine genuine balsam of Mecca, both when fresh, and after it is old. If the resinous substances abovementioned were not found in either, there would be reason to ascribe them to fraud: but it is probable, that they are natural to the balsam of Mecca, since Mr. Halle, who has had frequent occasions of dissolving this balsam in alcohol, always obtained the same result.

X.

Of the Fettstein: by Count Dunin Borkowski [*].

Character of the fettstein.

THE colour of this stone is sometimes seagreen, sometimes blueish. It is likewise found of a deep fleshcoloured red.

Externally it has but little lustre: internally it is very shining, with a greasy lustre. From this appearance its name, which signifies *fat stone*, is derived.

Its fracture is lamellar; not very determinate, though a double cleavage, with oblique junctures is perceivable; and a little scaly.

It is very translucid on the edges; hard, so as to strike fire with steel; and brittle.

Its specific gravity is 2·563.

Heated before the blowpipe its colour changes to an ashen gray; and it fuses, though with difficulty, into a white enamel.

It is found accompanied with feldspar and amphibole at Arendahl, in Norway.

When the fettstein is cut into a spherical form, it exhibits the same play of colours as the cat's-eye.

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[*] Journal de Physique, vol. lxix, p. 159.

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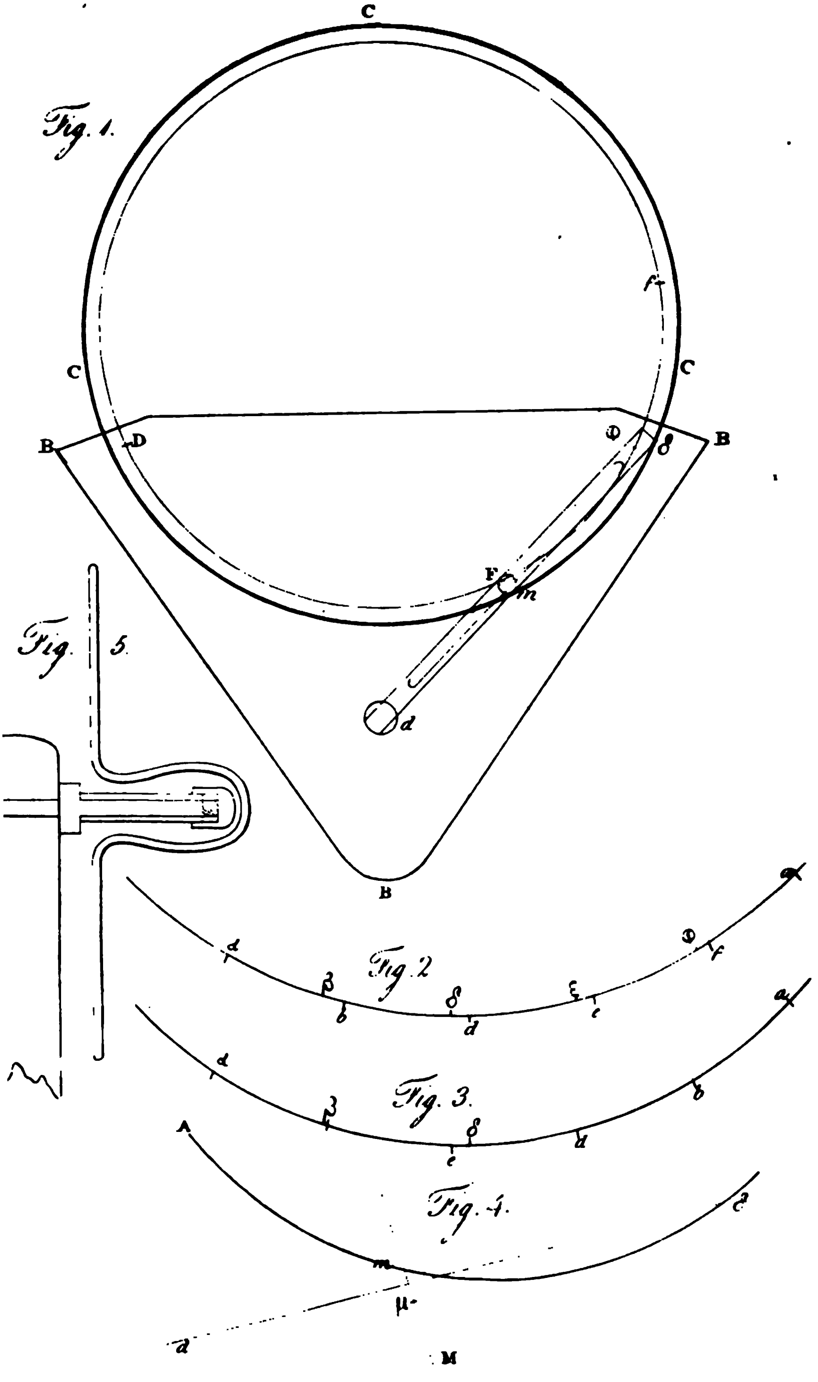

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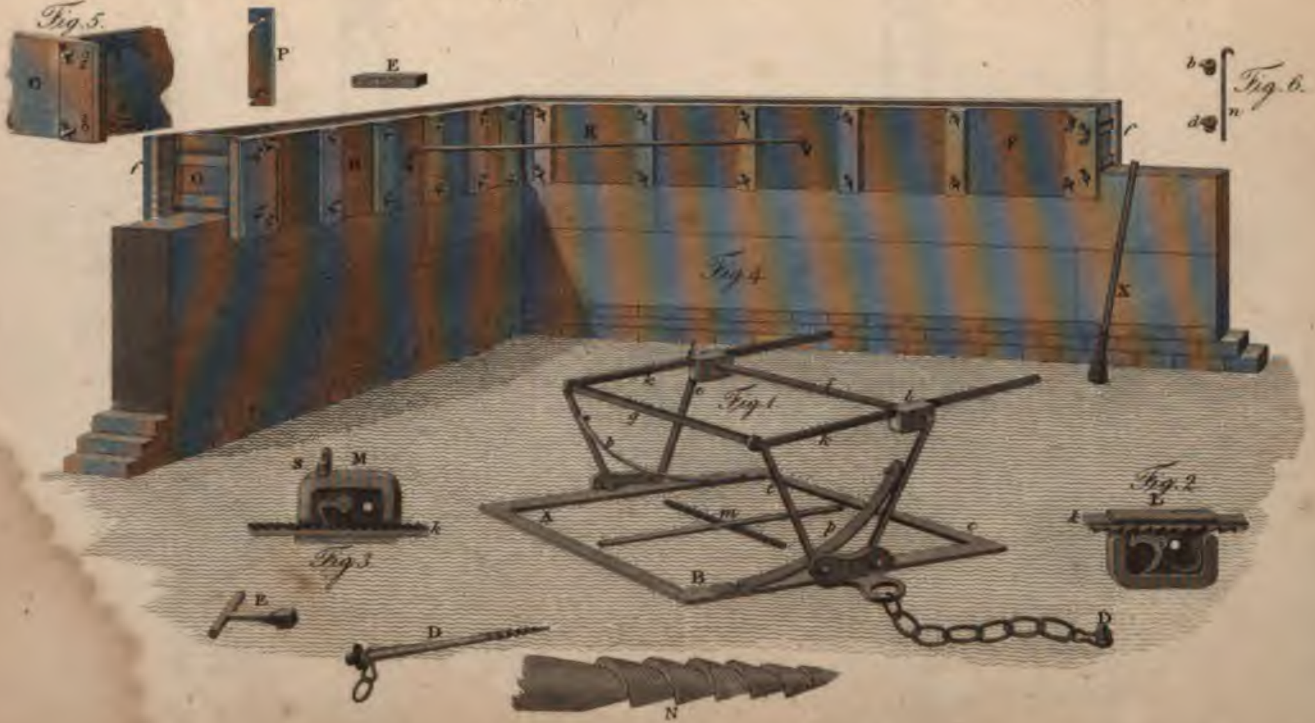

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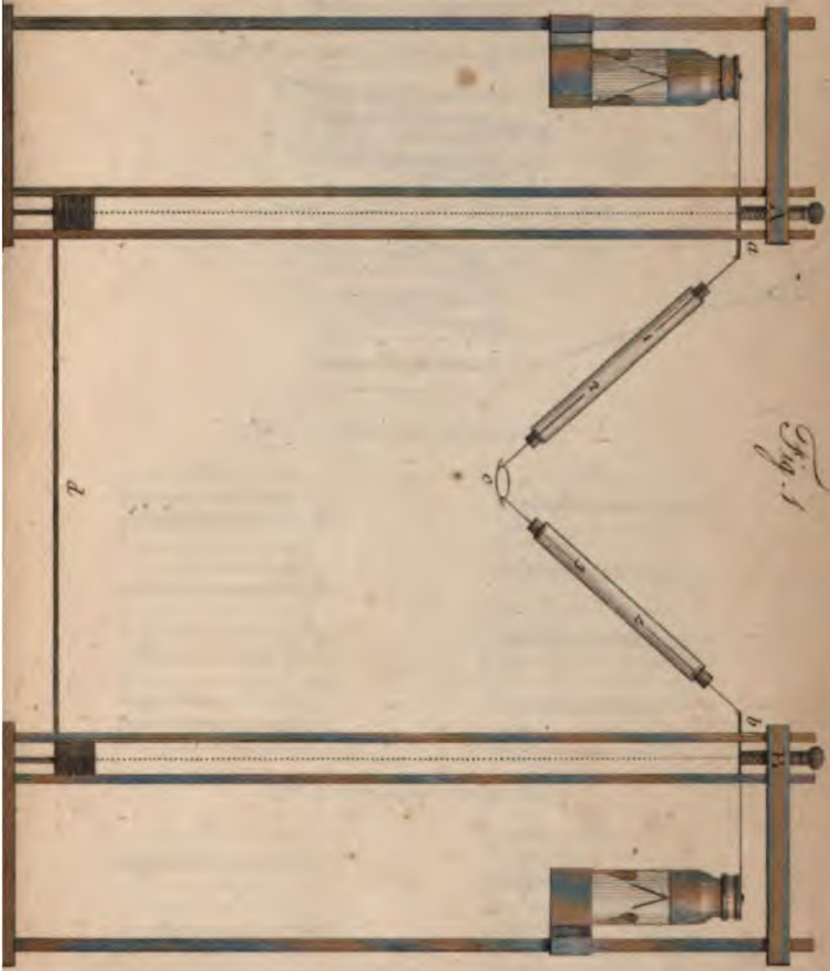

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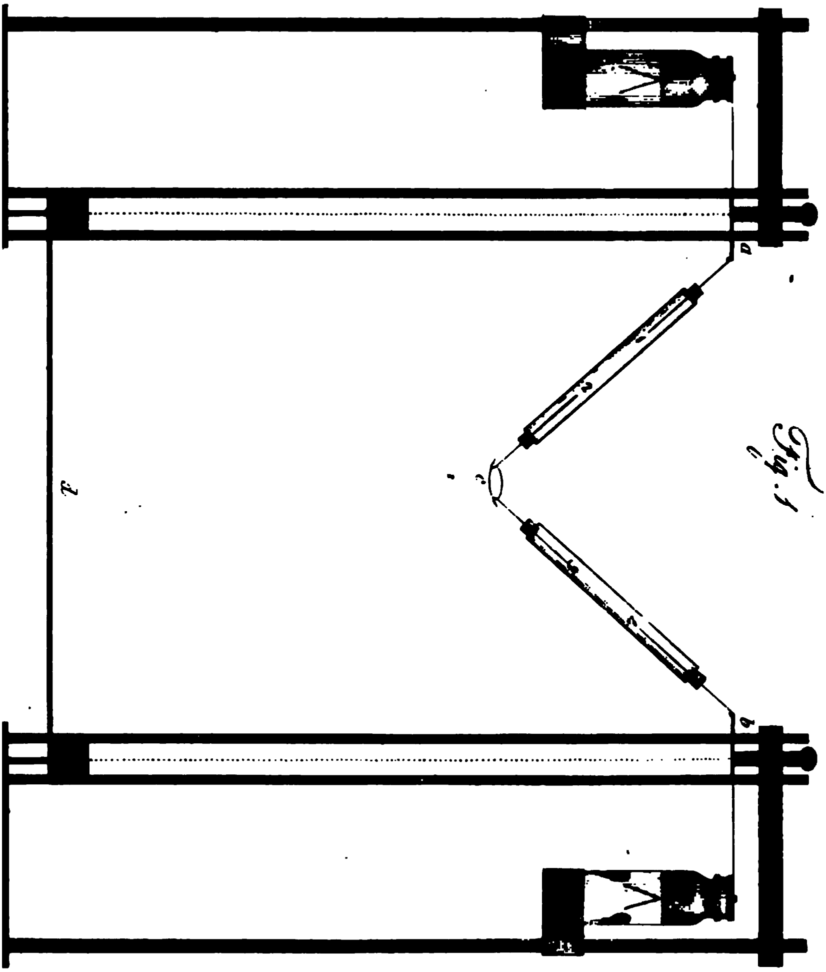

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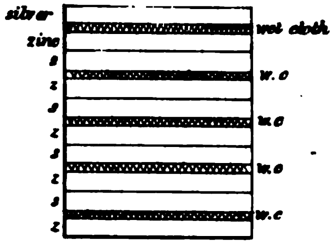

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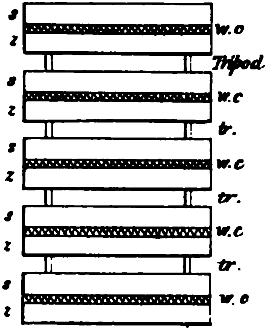

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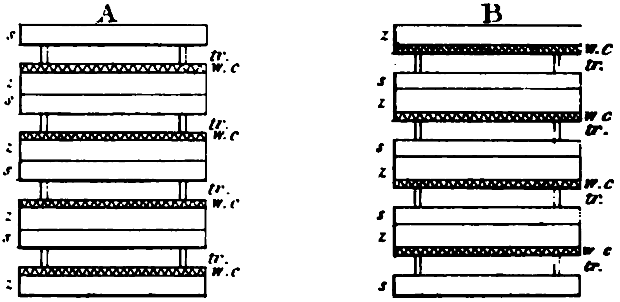

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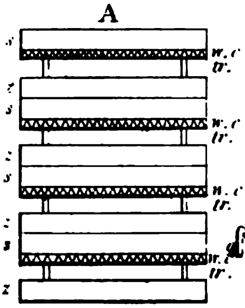

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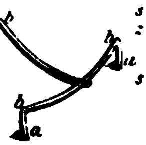

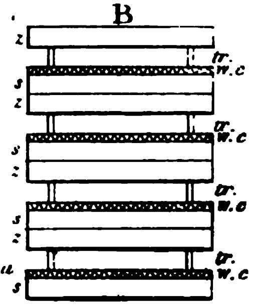

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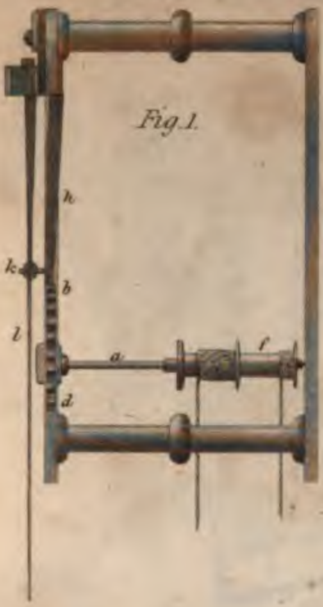
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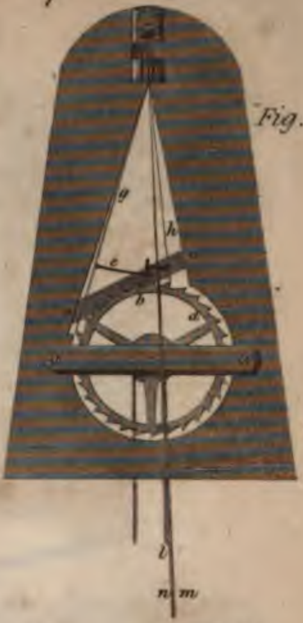
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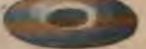
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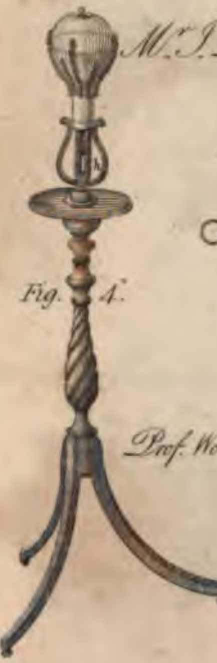
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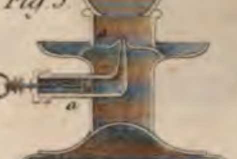
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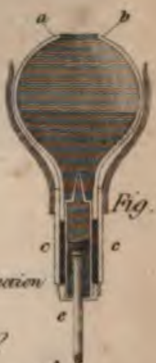
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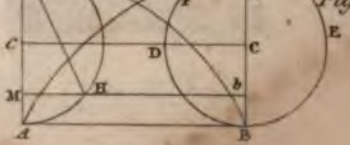
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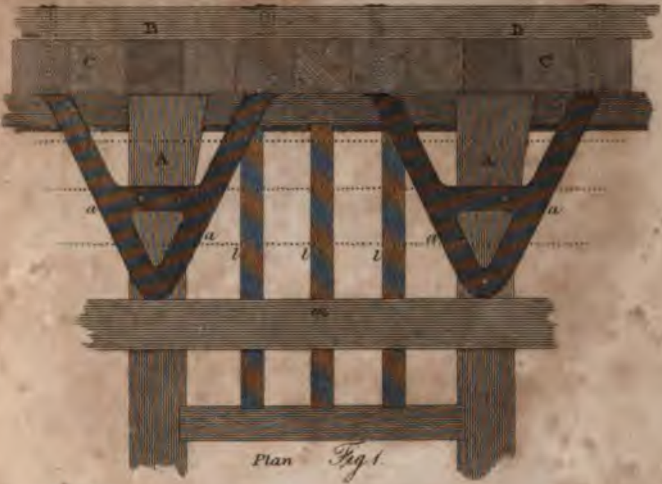

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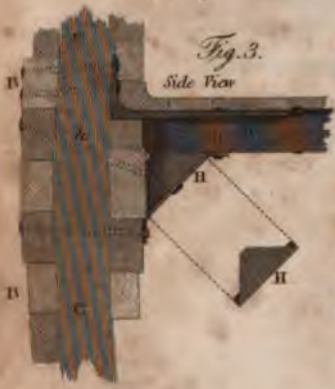

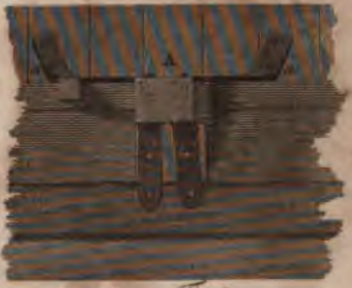

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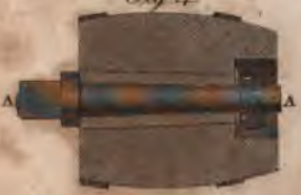

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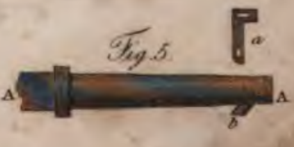

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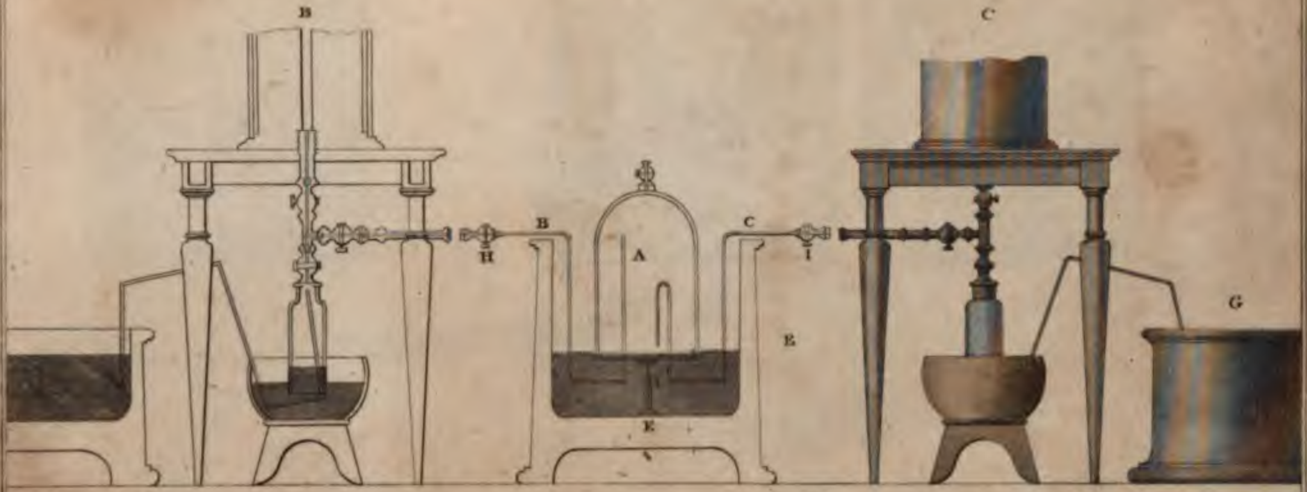

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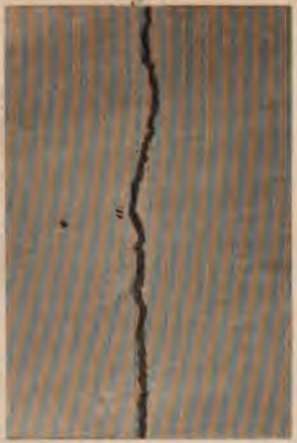

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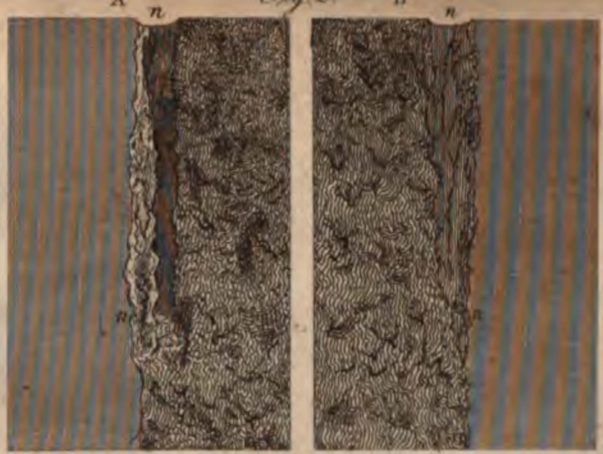

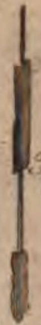

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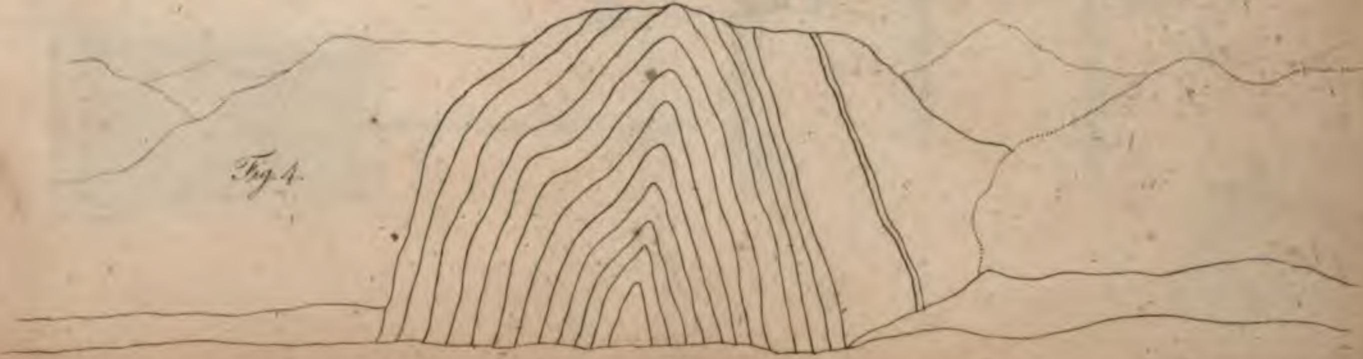

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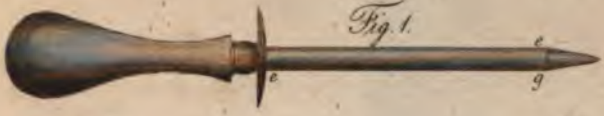
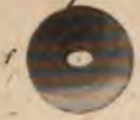


Fig. 1.

Fig. 3.


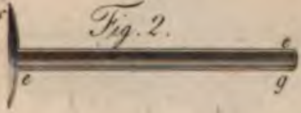


Fig. 2.

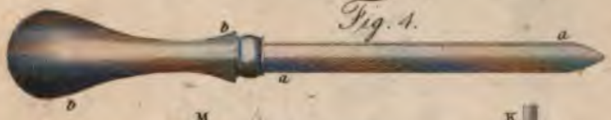


Fig. 4.

Mr. I. Fisher's improv'd Churn Staff

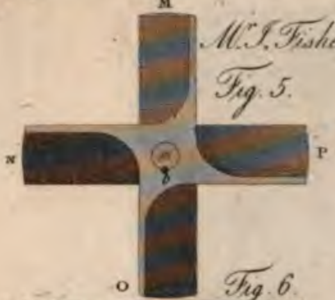


Fig. 5.

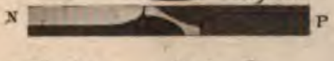


Fig. 6

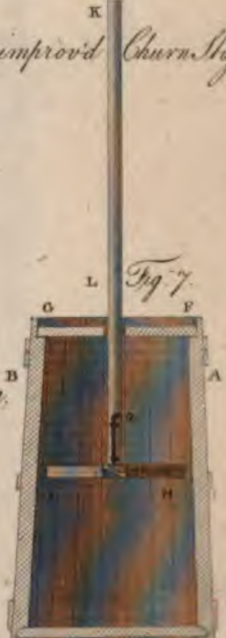


Fig. 7

Mr. Tansley's method of cutting Shag.

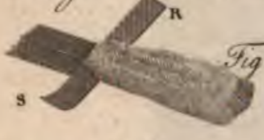
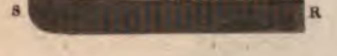


Fig. 8.

Fig. 9.


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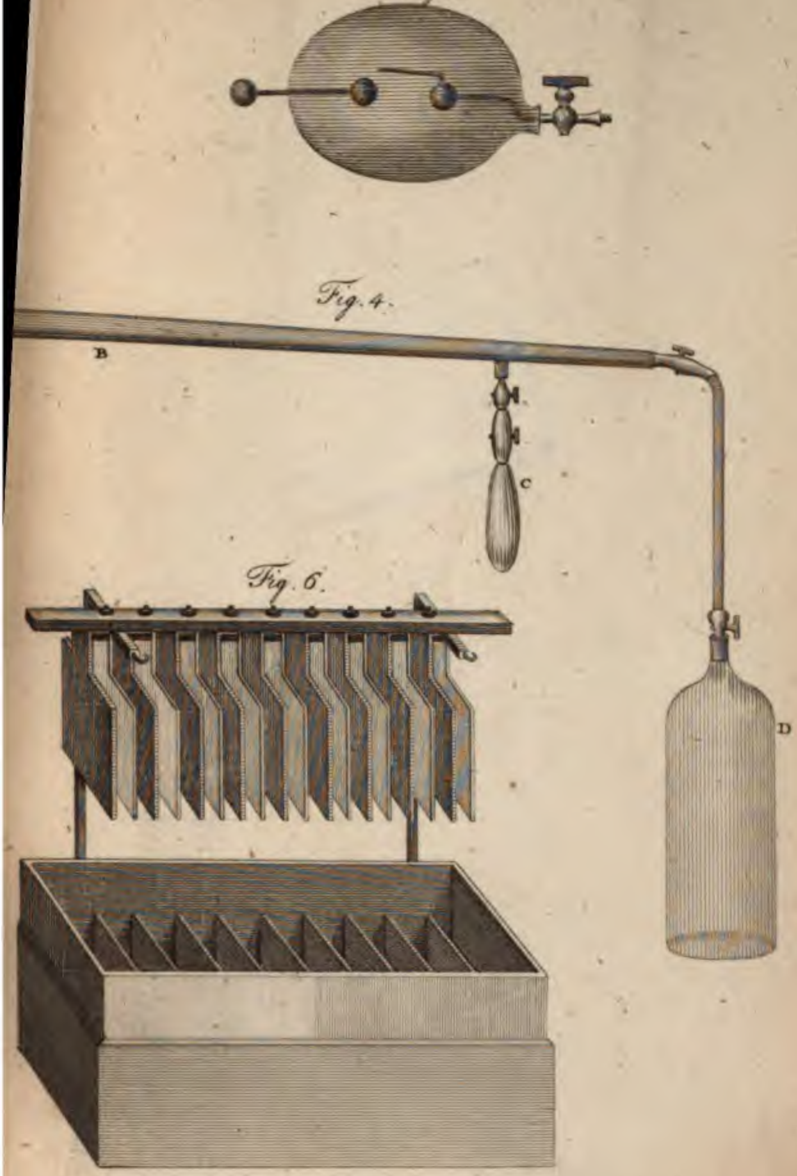

Fig. 5.

Fig. 4.

Fig. 6.

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Nicholson's Philos. Journal, Vol. XXVI. Pl. X. p. 323.

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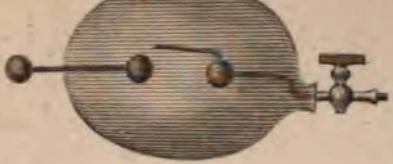

Fig. 5.

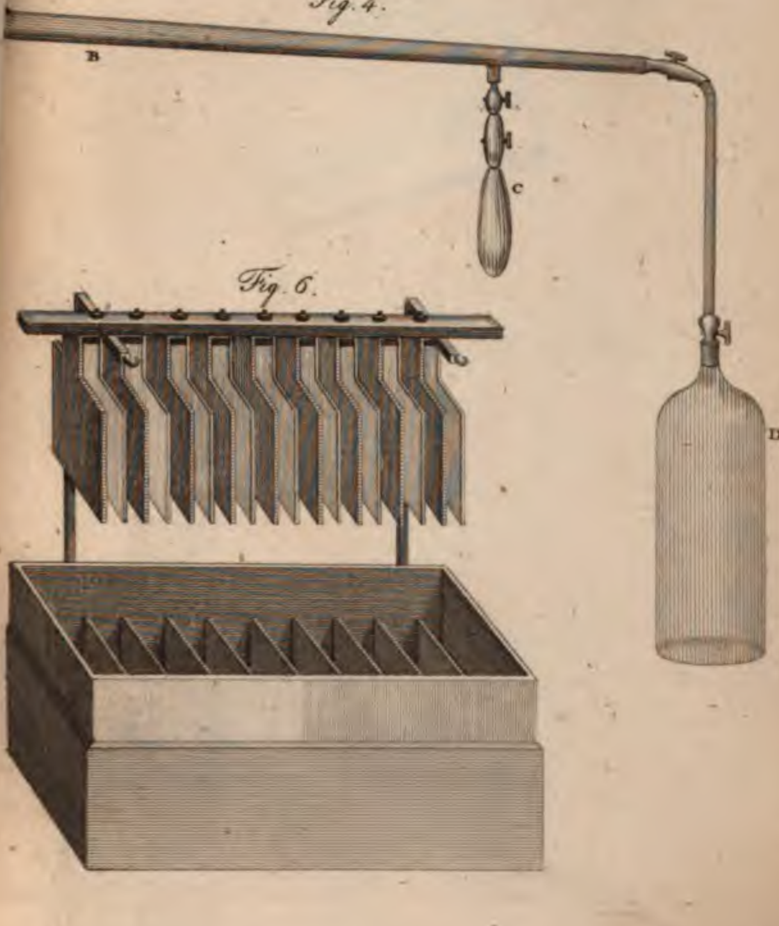

Fig. 4.

Fig. 6.

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